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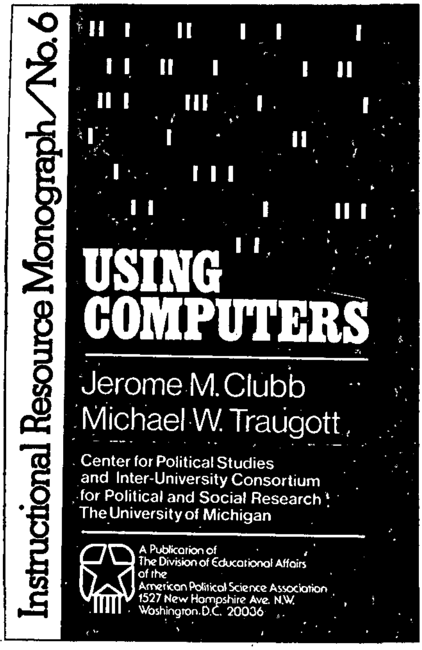

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Inducements versus Constraints: Disaggregating "Corporatism"

RUTH BERINS COLLIER
DAVID COLLIER
University of California, Berkeley

The concept of "corporatism" has usefully called attention to the importance of systems of interest representation based on non-competing groups that are officially sanctioned, subsidized, and supervised by the state. Yet these patterns have appeared in such a remarkable variety of political contexts that this concept may be too broad to be useful. On the basis of an analysis of the relationship between the state and organized labor in Latin America, this article argues that the concept of corporatism can be disaggregated so that it sheds light on rather than obscures the different power relationships and political contexts with which it is associated. The analysis focuses on the distinction between "inducements" extended by the state to win the cooperation of groups and "constraints" through which the state directly controls groups. This disaggregated approach enables one to distinguish more subtly among systems of group representation, to conceive of state-group relations in more interactive terms, and to gain insights into the larger political context.

The concept of "corporatism" has recently merged as a central point of reference in esearch on interest representation. More broady, this concept has played a central role in the enewed effort to discover more adequate ways f conceptualizing alternative patterns of state-ociety relations and alternative modes of poliical domination.[1] Scholars concerned with arious world regions have called attention to he tendency toward a corporative ordering of nterest politics and of state-society relations round non-competing groups which are offiially sanctioned, closely supervised, and often ubsidized by the state.

Corporatism has received particularly widespread attention in analyses of the relationship between the state and organized labor in Latin America. It is argued that Latin American governments have commonly sought to exercise control over labor movements and that within this context of control, the concept of corporatism captures an important aspect of the network of hierarchical relationships through which labor organizations come to be dependent upon and penetrated by the state.[2]

This focus on corporatism provides an important alternative to earlier pluralist perspectives in that it takes as a starting point the role of the state in shaping interest representation. Yet a closer examination of the contexts in which corporative patterns of state-labor relations have emerged in Latin America reveals such a diversity of political relationships and of goals on the part of elites who introduce corporative provisions that it seems reasonable to ask whether all these cases should be grouped under a single concept. While the concept of corporatism may be valuable as a first approximation, it appears to miss much of the give-and-take of politics.

We propose here a new approach to conceptualizing corporative patterns of state-group relations that will make it possible to deal more

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This article is part of a larger study of national olitical change in Latin America, portions of which ave been supported by National Science Foundation rant No. SOC 75–19990, the Social Science Research Council, and the Tinker Foundation. We are rateful to Lila Milutin, Leslie Spencer Herrera, Cherri Vaters, Richard Miller, Benjamin Most, and Dale Story or their assistance in the collection and preparation of he data. We also acknowledge the helpful comments f Philippe Schmitter, Louis Goodman, Ernst Haas, ohn Zysman, Carla Robbins, Edward Carmines, Ronld Weber, James Christoph, and Alfred Diamant.

[1] One indicator of the rising importance of this ocus is the appearance of special issues of two olitical science journals devoted exclusively to this opic: the January 1974 issue of *The Review of olitics* and the April 1977 issue of *Comparative olitical Studies*. Two of the most widely cited articles hat provide an overview of this theme are Wiarda 1973) and Schmitter (1974). A valuable recent iscussion is Stepan (1978).

[2] The focus on corporative structures as mechanisms of control is a central theme in the literature on Latin America (see footnote 5). However, it should be stressed that some forms of corporatism do not primarily involve state control over groups. The discussion of "state" versus "societal" corporatism at the end of this article addresses this issue.

adequately with this diversity of power relationships. This approach provides a better basis for analyzing contrasting patterns of state-group relations—including both differences among countries and patterns of change within countries. Though our immediate empirical referent is the relationship between the state and organized labor in Latin America, this modified conceptualization may be extended to the analysis of other types of groups and other regional contexts.

Corporatism

The term corporatism has been applied to a wide variety of phenomena, including modes of political participation, types of political action, ideologies, and broad cultural traditions (Wiarda, 1974; Rogowski and Wasserspring, 1971; and Palmer and Middlebrook, 1976). At the same time, there has emerged a common usage in which systems of interest representation—and more specifically, different patterns of state-group relations—are the central issue. Drawing on the shared usage in this literature,[3] one may define a system of state-group relations as corporative to the degree that there is (1) state *structuring* of groups that produces a system of officially sanctioned, non-competitive, compulsory interest associations; (2) state *subsidy* of these groups; and (3) state-imposed *constraints*[4] on demand-making, leadership, and internal governance. Corporatism is thus a *non-pluralist* system of *group* representation. In contrast to the pattern of interest politics based on autonomous, competing groups and to the total suppression of groups, in the case of corporatism the state encourages the formation of a limited number of officially recognized, non-competing, state-supervised groups.

This usage has been particularly common in research on state-labor relations in Latin America. Analysts have frequently viewed these relations as involving corporatism, with a widespread use of corporative mechanisms by the state to shape and control labor organizations.[5] And while there are unquestionably important periods in which state-labor relations in Latin America involve outright repression, as well as a very few cases of a fair degree of pluralism, one may accurately characterize the predominant pattern during much of the twentieth century as corporative.

At the same time, we have argued elsewhere (Collier and Collier, 1977) that in Latin America, corporative patterns of state intervention in organized labor have been introduced in the context of a striking diversity of power relationships and policy goals. Corporative provisions have been used in some cases to strengthen the position of workers and unions in relation to employers, whereas in others they have been used to weaken their position. Corporative provisions have sometimes been used by political parties to win workers' support, and at other times to insulate workers' associations from involvement with parties as a means of restricting their political power. In some contexts members of the military elite have seized the government and used corporative provisions to aid labor organizations and mobilize their support, whereas in others such elites have used these provisions to control labor sharply. Corporative provisions have been promoted by an extraordinary spectrum of governments, ranging from repressive, right-wing governments, through "populist" governments such as the Cárdenas government in Mexico and the first Perón government in Argentina, to Castro's Cuba (Wiarda, 1974, p. 4; Collier and Collier, 1977).

These observations suggest that the concept of corporatism may apply to so many different cases that it often tells one little or nothing. If such a diversity of cases can be found even within just one world region and considering only the relationship between the state and one class group, organized labor, perhaps this concept simply casts too broad a net to be useful.

There are two ways of avoiding this problem. The first is to treat corporatism explicitly as a dimension, or a set of dimensions, along which cases may be arrayed. Since "real pluralism" is relatively rare in the contemporary world, there may be a tendency to find corporatism almost everywhere. But, in fact, systems of interest representation are not identical everywhere; there are major differences in the degree of structuring, subsidy, and constraints introduced by the state. Corporative patterns of state intervention, like pluralism, should thus not be conceived narrowly as either present or absent, but rather as a variable that may assume different values, as a phenomenon that may be present to varying degrees (Collier and Collier, 1977).

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[3] Schmitter, 1974, pp. 93–94; Wiarda, 1974, p. 6; O'Donnell, 1977; Cotler, 1972; Schwartzman, 1977; Kaufman, 1977; Mericle, 1977; Stepan, 1978, Ch. 2; Reyna, 1977; Malloy, 1977.

[4] In Collier and Collier (1977) we referred to these types of restrictions as controls. For the sake of clarity, however, we will here refer to them as constraints and use the term control somewhat more broadly (see below).

[5] O'Donnell, 1977; Kaufman, 1977; Collier and Collier, 1977; Wiarda, 1976; Erickson, 1977; Harding, 1973; Schmitter, 1971, 1974; Mericle, 1977; Córdoba, 1974; Reyna, 1977; Corradi, 1974; and Petras, 1969.

The second means involves disaggregation. Because corporative intervention in interest representation appears in the context of very different relationships of economic and political power, one may reasonably ask whether the corporative patterns in these different contexts really are the same. Since few countries have the full complement of corporative provisions typically identified in the literature on this topic, perhaps different combinations of provisions appear in these different contexts. Though at a high level of aggregation these cases may all be corporative, at a more disaggregated level there may be striking differences among them.

Inducements versus Constraints

As a first step toward disaggregating corporatism, one may note that some corporative provisions bestow advantages upon the labor organizations that receive them, whereas others do not. The *structuring* of group representation through provisions that provide for such things as official recognition, monopoly of representation, and compulsory membership—as well as the *subsidy* of groups—provide important organizational benefits. In this sense these provisions are quite distinct from the *constraints,* which directly control labor organizations and labor leaders.

The idea that structuring and subsidy are benefits is supported by analysts of political organizations, who suggest that these provisions do in fact address basic organizational needs of labor unions (Bendix, 1964, pp. 80–97; Olson, 1971, Ch. 3; and Wilson, 1973, Ch. 3). These include the need to compete successfully with rival groups that seek to represent the same constituency; the need to be recognized as the legitimate representative of their constituency in their dealings with other sectors of society; the need to recruit and retain members; and the need for stable sources of income. Because structuring and subsidy help to meet these needs, they confer significant advantages on the organizations that receive them.

Though structuring and subsidy thus provide important organizational benefits, one must understand the political context in which these provisions appear in order to interpret their significance. Corporative policies toward organized labor in Latin America have been introduced from above by elites, acting through the state, who have used these policies to help them pursue a variety of goals—involving an effort to shape the behavior of the labor movement and/or to win its political support. It therefore seems appropriate, within the Latin American setting, to view structuring and subsidy not simply as benefits but as *inducements* through which the elite attempts to motivate organized labor to support the state, to cooperate with its goals, and to accept the constraints it imposes. In this context, corporatism may thus be viewed as involving an interplay between inducements and constraints.

This idea of an interplay between inducements and constraints is consistent with standard discussions of the dialectical nature of state-labor relations in Latin America. Goodman (1972, p. 232) has interpreted Latin American labor law, the most important formal expression of the relationship between the state and organized labor, as containing both a "carrot and a stick" for labor. Spalding (1972, p. 211) has analyzed the tendency of the state and elite groups in Latin America to "seduce and control" organized labor. The terminology employed in a standard manual of labor relations in the United States suggests that the inducement/constraint distinction is salient outside of Latin America as well. This manual contrasts provisions in labor law that involve "labor sweeteners" sought by unions with those involving "restrictions" on unions sought by employers (Bureau of National Affairs, 1972, p. 4). More broadly, Schmitter (1974, p. 92) hinted at this distinction when he suggested, without elaboration, that corporative provisions which we have referred to as involving constraints may be accepted by groups "in exchange for" the types of provisions we have identified as involving the structuring of groups.

Though one can thus distinguish between inducements and constraints, it is important to emphasize that these are not diametrically opposed phenomena. Analysts of power and influence such as Lasswell and Kaplan (1950, pp. 97–98) and Gamson (1968) distinguish between inducements and constraints but view both as mechanisms that serve to influence behavior. Constraints are seen as producing compliance by the application, or threat of application, of negative sanctions or "disadvantages." Inducements, by contrast, involve the application of "advantages" (Gamson, 1968, pp. 74–77). Yet in this literature inducements are also viewed as mechanisms of co-optation. As such, though they involve "advantages," they also lead to social control.

This dual nature of inducements is clearly evident in the specific mechanisms of structuring and subsidy discussed above. These inducements may, like the constraints, ultimately lead to state penetration and domination of labor organizations, for at least three reasons. First, an inducement such as monopoly of representation is by its nature offered to some labor organizations and withheld from others. This provision has commonly been used in Latin America to undermine radical unions and to promote those favored by the government (Harding, 1973, p. 71; Kenworthy, 1970, pp. 159–60; and Silverman, 1967, pp. 137–54). Second, unions receiving inducements must commonly meet various formal requirements in order to receive them. Finally, the granting of official recognition, monopoly of representation, compulsory membership, or subsidy by the state may make the leadership dependent on the state, rather than on union members, for the union's legitimacy and viability. This dependency accelerates the tendency for labor leadership to become an oligarchy less responsive to the needs of the workers than to the concerns of state agencies or the political elite with which the leaders interact. This dual nature of the inducements explains why high levels of inducements, as well as of constraints, are often instituted by members of the elite whose goal is to produce a docile, controlled labor movement.

If both inducements and constaints ultimately lead to social control, it remains to be demonstrated that labor organizations really desire to receive the inducements—that these provisions in fact induce labor organizations to cooperate with the state and to accept the constraints. A preliminary examination of the evidence suggests that this is often the case.

For example, these assessments are often expressed at the time of the enactment of the first major law that provides a basis for legalizing unions and that commonly includes a number of inducements and constraints for the unions which become legally incorporated. An important example is found in Argentina. The dominant sector of the Argentine labor movement initially rejected Juan Perón's initiatives to gain the cooperation and support of the labor movement in the 1940s. Only when Perón began to adopt the program of this sector of the movement, i.e., to support the *organizational* goals of labor as well as their substantive demands on bread and butter issues—in part through a labor law that placed heavy emphasis on inducements—did major sectors of the labor movement begin to accept his offers of cooperation (Silverman, 1967, pp. 134–35).

In Mexico the reaction of labor to the first national labor law in 1931 again reflected the dual nature of the law, encompassing both inducements and constraints. Labor leaders objected to certain constraints—the provisions for federal supervision of their records, finances, and membership lists—whereas they accepted the provisions for the recognition of unions—defined above as an inducement. Furthermore, they were dissatisfied over the *absence* of a provision that is clearly an inducement—compulsory membership (Clark, 1934, p. 215; Harker, 1937, p. 95).

The debate within the labor movement concerning the passage of the 1924 labor law in Chile reflects this same pattern. The dominant Marxist sector of the labor movement generally accepted the new system, arguing that it had to "use all the social legislation of the capitalist state to fight capitalism itself" (Morris, 1966, p. 246). The debate within the labor movement showed that though this sector opposed the constraints contained in the law, it was clearly attracted by the law's provisions that would help it to extend its organization to new economic sectors and would allow it to receive a state-administered financial subsidy derived from profit-sharing. The inducements contained in the law were thus sufficient to motivate the dominant sector of the labor movement to cooperate with the state.

The 1924 Chilean law is useful for underlining another point as well. Though the inducements offered by the state have often been sufficient to win the cooperation of labor, this has not always been the case. Historically, the anarchists were acutely aware not only of the costs of the constraints that accompany the inducements, but also of the tendency of the inducements themselves to lead to social control. Thus, following the traditional anarchist position regarding the risks of co-optation arising from cooperation with the state, the anarchist sector of the Chilean labor movement rejected the 1924 law completely. Another example is the 1943 law in Argentina, which was widely opposed by organized labor. At that point, the state was not willing to extend sufficient inducements to win the cooperation of labor, which rejected the constraints. It is noteworthy that the Peronist law of 1945 (see above) provided the necessary level of inducements and was accepted by organized labor, despite its similarly high level of constraints.

These examples suggest that while some groups will resist these inducements, the inducements have, in fact, often served to win the cooperation of labor groups and to persuade them to accept the constraints. Furthermore, the distinction between inducements and constraints is not merely an analytic point of concern only to social scientists, but rather a vital political issue in the history of state-labor relations in Latin America.

Different Political Contexts

Do inducements and constraints occur in distinct patterns in different political contexts? If so, does the analysis of these different patterns contribute to understanding the different settings in which corporatism has appeared? To address these questions, we must focus in greater detail on specific corporative provisions that have typically characterized state-labor relations in Latin America.

An Operationalization of Inducements and Constraints. In the context of state-labor relations, inducements and constraints of course take many forms–including bribery and overt repression. Yet many important inducements and constraints–such as those conventionally referred to in conceptual discussions of corporatism–are found in labor law, which will be used here as a basis for illustrating the interplay between these two dimensions. We hardly need emphasize that law does not, by itself, reflect the full reality of state intervention in labor organizations or labor relations. Laws may not be applied, or they may be applied differentially.

Yet law is important. It is commonly asserted by specialists in organized labor in Latin America that labor law is indeed one of the crucial factors that shape labor movements (Miller, 1966, p. 11; Erickson, 1977, p. 29; Wiarda, 1976, p. 11; International Labor Office, 1961, p. 269; and Valenzuela, 1976, p. 151). Furthermore, the adoption of laws is a major step in the decision process through which state intervention in labor representation crystallizes. Labor law is a highly visible and concrete policy statement around which political battles are fought, won, and lost, and around which political support is attracted, granted, and withheld. Especially for the years in which labor law is promulgated or modified, law thus provides a valuable point of reference for analyzing the larger political context. Of course one must be cautious in using an older law which has been left on the books as a basis for interpreting the politics of a subsequent period. We therefore focus particularly on the years in which laws are adopted–though in some cases, as in the interpretation most notably of Mexico, long periods of stability of law do point to an important continuity in the political context. Despite this caveat, however, law provides a useful source of data for the comparative analysis of the different approaches to shaping labor organizations and labor relations that are grouped together by policy makers in this crucial phase of the policy process. For the analyst concerned with whether different patterns of inducements and constraints appear in different political contexts, law thus represents a valuable source of data.

In order to apply the inducement-constraint distinction in a comparative/longitudinal analysis of state-labor relations in Latin America, we scored a series of legal provisions corresponding to the different elements in standard definitions of corporatism discussed above for 20 Latin American countries for each year over the period 1901 to 1975.[6] Under the heading of inducements, the scoring focused on provisions regarding registration, right of combination, monopoly of representation, compulsory membership, and subsidy of unions. The heading of constraints included provisions regulating collective bargaining and strikes, other controls on demand-making, controls on leadership, and provisions for state monitoring and intervention in internal union affairs. While these provisions obviously do not include all inducements and constraints that may appear in labor law, they represent a constructive starting point for analyzing this distinction.[7] Statistical analysis of these provisions indicated that it was appropriate to group them into two overall scales that reflected the degree to which inducements and constraints were present in the labor legislation of each country. These scales are used as the basis of the analysis presented below. Appendices A and B explain the scoring of these legal provisions and the construction of these scales.

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[6] We used International Labor Office 1919–1975 and 1930, as well as a wide variety of other secondary sources, for identifying relevant statutes and for scoring the statutes. For most of the statutes, the original text was consulted as well.

[7] The scoring was restricted to labor law as it is formally defined in Latin American legal systems. Though the discussion does at a few points take into consideration other major legal provisions, such as states of siege, that may supersede labor law, the formal scoring does not attempt to cover all provisions relevant to labor organizations. It does not, for instance, consider provisions contained in criminal codes in the earlier part of this century that were used to restrict labor organizations in the period before the advent of formal labor legislation. The goal is not to provide a definitive assessment of all legal provisions relevant to labor organizations, but rather to see if certain patterns emerge among the provisions that have particularly concerned analysts of corporatism.

Contrasting Patterns of Change. The distinct patterns of change in inducements and constraints in Figure 1 provide a useful starting point for exploring the relationship between these dimensions and the larger political context. In Argentina, for instance, one finds a volatile pattern of change that reflects frequent shifts in the coalitional position of organized labor in Argentine politics—as well as, overall, a relatively greater emphasis on inducements in relation to constraints.[8] The dramatic shift to a high level of inducements in 1945 has already been noted. The context of this shift was the effort by Perón to gain the support of the large, well-established, and autonomous Argentine labor movement as he attempted to rise from a subordinate position within the military government that came to power in 1943. With the help of labor support, he was elected president in 1946. One of Perón's most visible acts in his attempt to court labor support was the abrogation of the unpopular 1943 law, which was heavily oriented toward constraints. In 1945 a Peronist labor law was introduced which included a similar level of constraints but which attracted overwhelming labor support in part because of the high level of inducements. Though Perón's most pro-labor period might be said to have ended in 1946 and though Perón became increasingly preoccupied with curbing labor and its demands as early as 1948–49, this policy shift proved quite difficult, since Perón remained heavily dependent on labor support. This combination of dependence on labor and concern with restraining its demands is reflected in the more "balanced" addition to labor law in 1953. The years following the ouster of Perón in 1955 were characterized by ongoing shifts in labor law that correspond to changes in the political context. For instance, in 1956 the anti-Peronist government which sought to undermine the dominant Peronist segment of the labor movement added constraints and dropped inducements. In 1958 inducements increased and constraints decreased as Peronists bargained with Frondizi over the terms under which they would grant him their electoral support.

The relationship between the state and organized labor in Brazil has been different from that in Argentina, and a different pattern of labor law has evolved.[9] Organized labor has been relatively weak in Brazil and played a marginal role in the rise to power of Vargas in 1930. In fact, a major concern of the leaders of the "Revolution" of 1930 which brought Vargas to power was to preempt the emerging labor movement and the "Bolshevik threat" which its connection with the Communist party seemed to imply. Once in power, Vargas sought to dismantle this labor movement and replace it with a state-controlled system of labor representation. Though his labor and welfare policies eventually won him the support of much of the working class, Vargas was not dependent on the working class for political support in the way that Perón was in Argentina. Correspondingly, within the framework of a more full-blown corporatist system, Brazil moved to high levels of both inducements and constraints.

Within this overall pattern, there are interesting short-term changes in Brazil. The rise in influence of anti-corporative, liberal groups in the mid-1930s is reflected in a brief reduction in the level of inducements. The earlier level was restored in 1937, and by 1939, under the explicitly corporatist Estado Novo (New State), Brazil moved to an even higher level of both inducements and constraints. By 1943 the Estado Novo was on the defensive and Vargas began laying the groundwork for the more active electoral support that he would need with the introduction of competitive politics after 1945. At this point he assumed a more populist stance, sponsored a political party to mobilize labor support, and introduced a more inducement-oriented labor law. The period following the fall of the Estado Novo in 1945 was characterized by shifting power relationships which produced, as in Argentina, a "circular" pattern of change in law (see Figure 1) as provisions for inducements and constraints were promulgated and abrogated. For instance, after the fall of Vargas in 1945 there was a brief reduction of inducements as the new government sought to undermine the position of the dominant sector of the labor movement, which was linked politically to Vargas. These provisions were restored within a few months in the face of protests from labor leaders.

The link between the larger political context and the pattern of inducements and constraints is clear in other cases as well. In Mexico, the labor movement has since an early phase of the Mexican Revolution been an important, though co-opted, actor within the dominant national coalition (Brandenburg, 1964; Everett, 1967; Stevens, 1974; Purcell, 1975). Correspondingly, the first major national labor law in 1931 placed heavy emphasis on inducements. Since then, labor law, like the dominant political coalition itself, has been relatively stable. The clearest case of a separate elaboration of constraints and inducements and of a dramatic "right-angled" shift from one to the other is Peru. Until the late 1950s, Peruvian labor law had been shaped by a series of conservative

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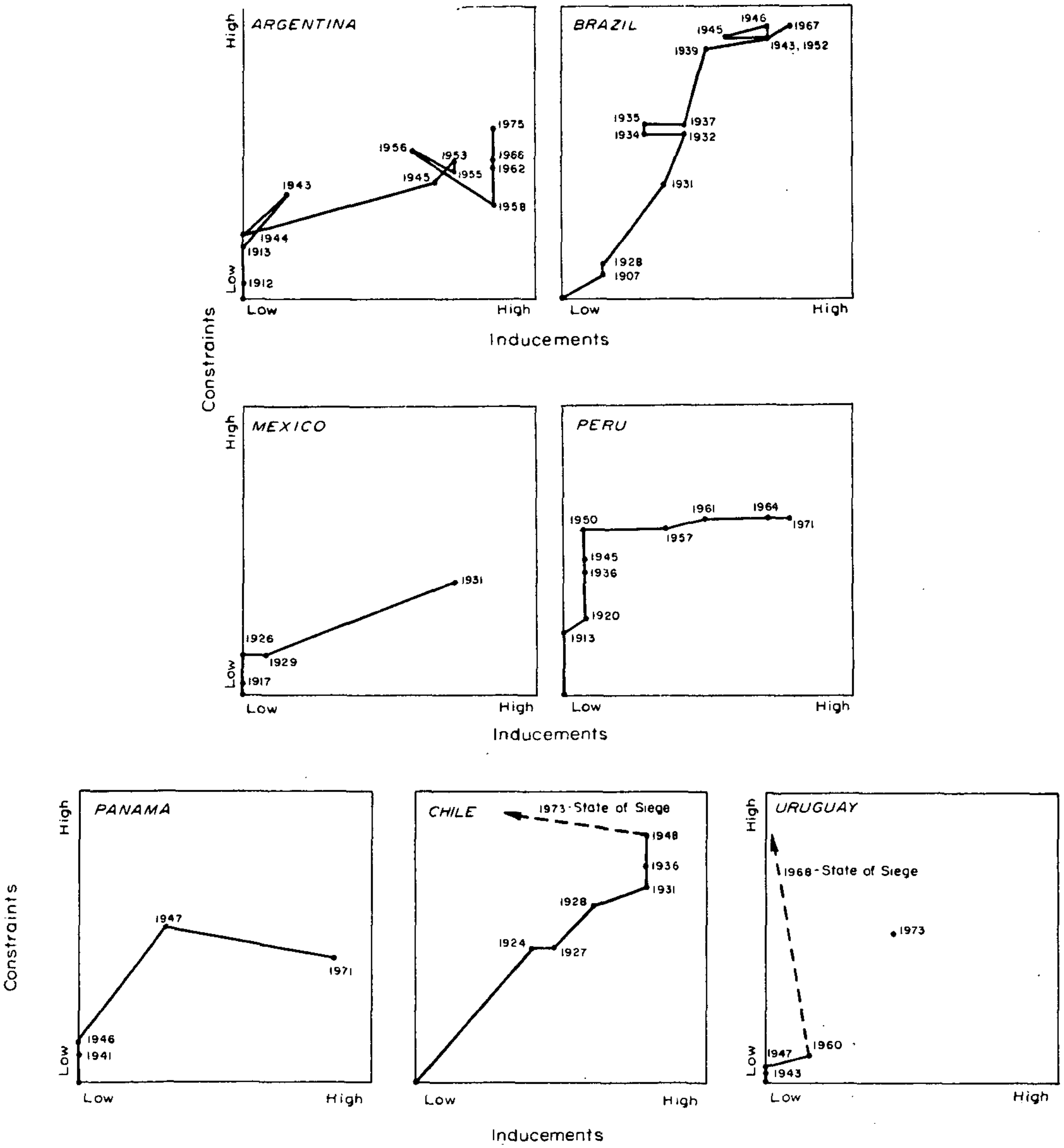
[8] This discussion of Argentina draws on Kenworthy, 1970, Ch. 5; Silverman, 1967, pp. 134 ff., 194–210, 221; Baily, 1967, Ch. 7; and Most, 1978, Ch. 4.

[9] This discussion of Brazil draws on Skidmore, 1967; Harding, 1973; Schmitter, 1971; and Erickson, 1977.



Source: Scored on the basis of data derived from I.L.O., 1919–1975 and 1930; other I.L.O. sources; and the statutes of individual countries.

Figure 1. Patterns of Change in Inducements and Constraints, 1901 to 1975

governments concerned with curbing the powerful, labor-based Apra party. Labor law was heavily oriented toward constraints, and the 1950 law in Peru was nearer the "high constraint/low inducement" corner of the diagram than any other labor law in Latin America.[10] Starting in 1957, after Apra entered into an alliance with the Peruvian elite, there occurred a series of increases in inducements, with little further increase in constraints. Another case of a particularly dramatic shift to the inducement side is Panama, which until the 1970s had a constraint-oriented law. In 1971 the populist/nationalist government of Torrijos made a strong appeal for labor support (*Latin America Political Report*, 1970–1971, passim) and promulgated a law which decreased the level of constraints at the same time that it increased inducement provisions to a level as high as any in Latin America.

Though the recent history of Chile saw the emergence of a powerful political left that enjoyed crucial support from the working class, the standard interpretation of the earlier history of state-labor relations emphasizes the preemptive, co-optive role of the state in attempting to create a weak, dependent labor movement (Morris, 1966; Peppe, 1971; Angell, 1972; and Valenzuela, 1976). Correspondingly, Chile—like Brazil—followed in this earlier period a relatively balanced pattern of inducements and constraints and moved to a high level of each. In 1973, with the fall of Allende and the onset of the violently anti-labor policies of the military government, the existing system of inducements and constraints was superseded by a state of siege and a variety of other legal and extra-legal measures, and Chile shifted to a system that predominantly involved repression, with little use of co-optation during the first years of military rule (I.L.O., 1975). This in effect involves an extreme movement upward and to the left in the diagram. Another example of a dramatic shift of this type is Uruguay. After many years as the most pluralistic system in Latin America with few legal provisions relating to labor organizations, Uruguay adopted in 1973, in the midst of a severe political crisis, a law that had a rough balance of inducements and constraints within the framework of a relatively low overall level of corporatism. However, state-labor relations have in fact been governed almost continuously during the violently anti-labor period since 1968 by the legal framework of a state of siege (Handelman, 1977, p. 11).

Comparisons across Countries. The comparison of major laws in Argentina, Brazil, Chile, Colombia, Mexico, and Venezuela sheds further light on the relationship between the larger political context and different patterns of inducements and constraints.[11] Four of these laws (1924 in Chile, 1931 in Brazil and Colombia, and 1936 in Venezuela) were for each country the first major labor law that legalized the trade union movement. These laws were adopted by governments that might be called "conservative modernizers." In these cases, a multi-class, modernizing coalition came to power, but organized labor did not play a critical role in building the coalition or providing support for it. Instead, the coalition derived its legitimacy largely from other sources. These labor laws tended to be the vehicle through which the government addressed the "social question." These governments sought to limit the radicalization of the working class by addressing themselves to the worst abuses to which this class was subjected and by seeking to integrate labor into the established order within a framework of substantial emphasis on constraints. This grouping includes the laws promulgated under the Liberal party in Colombia, which in 1930 came to power after a long period of Conservative party rule; López Contreras in Venezuela, who came to power after Gómez's repressive, dictatorial rule; and Vargas in Brazil and Alessandri in Chile, both of whom came to power at a point when traditional oligarchic rule had broken down and become discredited.

These four laws have relatively similar levels of inducements and constraints (see Figure 2). The laws did not contain as many provisions for either inducements or constraints as the later laws, and like many early laws placed a greater relative emphasis on constraints than on inducements. Hence they are located above (i.e., on the constraint side of) the hypothetical line of relative "balance" in Figure 2 (see note at bottom of figure).

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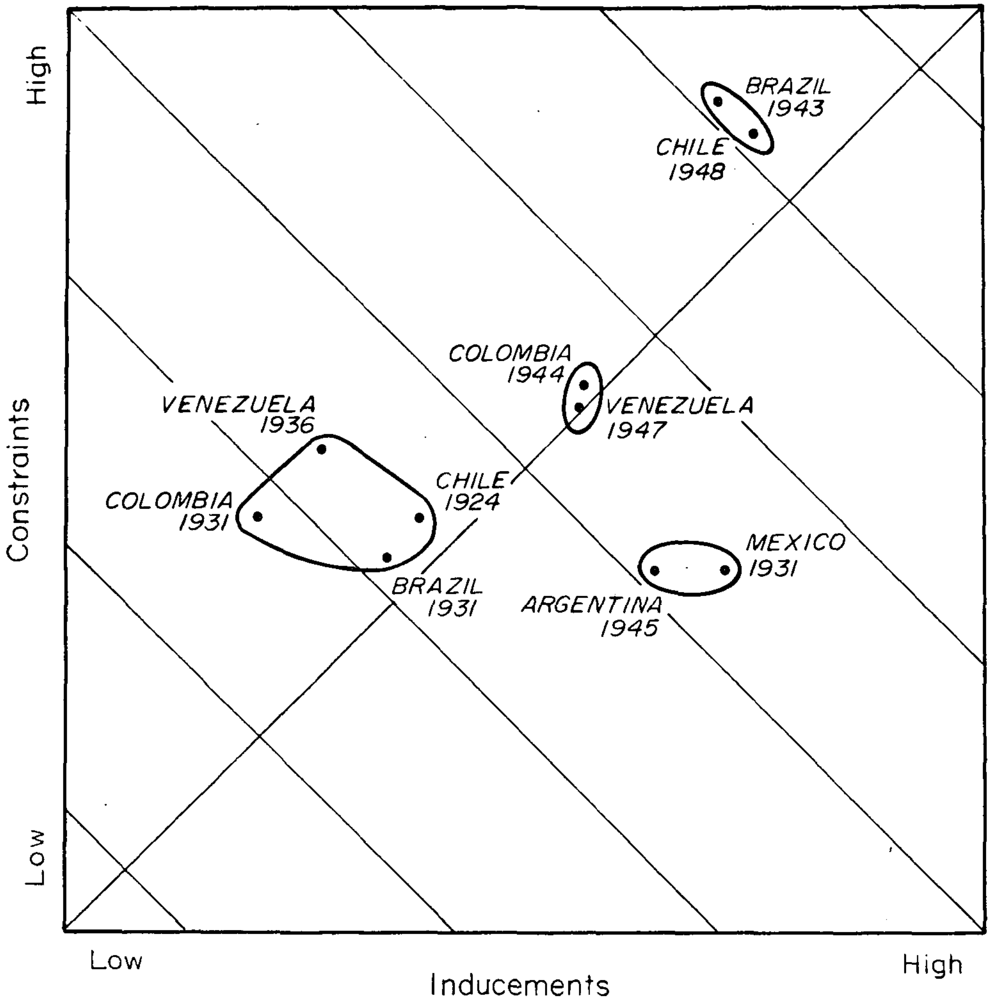
[10] This discussion of Peru draws on Sulmont, 1975, pp. 188–89, 239; Bourricaud, 1967; and Payne, 1965.

[11] We selected the larger, more industrially advanced countries of Latin America, in part because they represent a coherent group and in part because we have analyzed them closely elsewhere (see R. Collier, 1978; and D. Collier, 1978). In addition to the sources cited above, the discussion in this section draws on Ashby, 1967; Blank, 1973; Brown, 1964; Caicedo, 1971; Cornelius, 1973; Dix, 1967; Drake, 1971; Fluharty, 1957; Imaz, 1967; Levine, 1973; Martz, 1966; Michaels, 1966; Nunn, 1970; Powell, 1971; Ruiz, 1976; Spalding, 1977; Stevenson, 1942; and Urrutia, 1969.



Source: Scored on the basis of data derived from I.L.O., 1919–1975 and 1930; other I.L.O. sources; and the statutes of individual countries.

*Parallel lines from upper left to lower right correspond to equal levels of inducements *plus* constraints. In one sense, they could thus be interpreted as reflecting the "over-all" level of corporatism. The single line from lower left to upper right can be used as a basis of reference in interpreting the relative "balance" between inducements and constraints. This is not to say that laws which are close to this line are in some substantive sense "balanced." Rather, comparing laws in terms of their distance from this line provides a basis for assessing the *relative* balance between inducements and constraints.

Figure 2. Inducements and Constraints in Selected Major Laws*

The second group of cases includes Colombia and Venezuela at a subsequent point in time (1944 and 1946, respectively). Here the labor movement was also weak, but the activation of labor played a more central role in legitimating the dominant coalition. These "populist" coalitions were put together from above by Acción Democrática in Venezuela and the pro-labor wing of the Liberal party in Colombia. Both of these parties sought to create and mobilize an organized labor sector, and both were dependent on labor as an essential support group for the government. Correspondingly, the new laws in both countries involved primarily the addition of new inducement provisions, and both countries moved from their earlier position in

Figure 2 over to the "balance" line, reflecting a greater relative emphasis on inducements in relation to constraints. At the same time that the addition of inducements is consistent with the greater dependence of these governments on labor, their level of constraints is consistent with the fact that these are far from radical, labor-dominated governments. Rather, they were multi-class coalitions dominated by middle-sector groups who needed the mobilization and support of labor.

The most inducement-oriented group includes Argentina and Mexico, the two countries which had the strongest labor movements when the major laws were promulgated. While the populists in Colombia and Venezuela were trying to create a labor movement which would form a support group for the party, in Mexico and Argentina the task was to gain or sustain the support of an already existing and relatively powerful labor movement. The early political strength of organized labor in Mexico grew out of the role of the Red Batallions in the Mexican Revolution, the ideology and expectations that derived from the 1917 Constitution, and the subsequent role of labor as a major, though co-opted, support group for the early revolutionary governments. The strength of Argentine labor derived from quite different sources. In this case, the major law came late—both chronologically and, even more so, relative to the level of industrialization. As a result, when Perón appeared on the scene, he faced a labor movement which, though subjected to repression, had for many years been developing autonomous associations. Correspondingly, the legal relationships which emerged in these contexts in which organized labor was relatively strong were different from those in the other four countries. One finds relatively low levels of constraints and high levels of inducements.

The final group includes Brazil and Chile, which started out—along with Venezuela and Colombia—in the group of conservative modernizers. Unlike these latter two countries, however, Brazil and Chile did not subsequently have comparable populist periods in the 1940s.[12] Instead, they continued and further elaborated their earlier efforts to co-opt and control the labor movement. Correspondingly, in terms of both inducements and constraints, Brazil and Chile had by the 1940s moved to the highest levels in the region.

Identifying Patterns. On the basis of these findings, it is possible to identify recurring patterns in the relationship between the political context and different combinations of inducements and constraints. Salient features of the political context include the degree of elite concern with winning the political support of organized labor, the degree of concern with controlling labor, and the strength and autonomy of the labor movement.

It appears that a higher level of inducements and a lower level of constraints tend to occur in contexts in which the government seeks to gain or retain the political support of labor and in which unions are relatively powerful and/or autonomous. In these cases labor has a greater capacity to resist the imposition of constraints and/or the state has a greater need to extend inducements in order to gain the support and cooperation of labor.

A higher level of both inducements and constraints is more likely in contexts in which the government is less concerned with gaining labor's support and more concerned with controlling labor through creating organizationally viable unions that are co-opted by and dependent on the state. This is often done to preempt the emergence of autonomous unions that are not dependent on the state.

A higher level of constraints combined with a lower level of inducements tends to appear in contexts in which the primary concern of the government is with control, to such an extent that it does not seek even the passive support from organized labor that it may receive in the other cases and does not mind risking the outright opposition of labor. Rather than relyng on co-optation, this control is based primarily on direct constraints on unions and is acked by considerable force and repression. This pattern is seen in contexts in which labor s strong, as when an extremely anti-labor overnment attempts to deactivate and impose evere controls on a highly developed labor novement.

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[12] The analysis of Brazil presented above referred to a shift to more populist policies in 1943. However, this was hardly comparable to these periods in Venezuela and Colombia. Chile likewise had a certain type of populist period in the form of the Popular Front government which came to power in 1938. What distinguishes Brazil and Chile from the other cases is the absence of a major mobilization of popular sector groups by a centrist party. In the other countries, the mobilization of the popular sector culminated in a period of "radical populism" in which the interests of the popular sectors were championed and the conservative elements (major elite economic interests) within the party became alienated and went into opposition. The result of such populist periods was polarization along class lines. In Brazil and Chile the subsequent "populist" periods (at least through the 1950s) were quite different, involving a coalition of parties based on agreement among party leaders rather than a popular sector mobilization by the dominant party. As a result, the party did not move to the left in the same way, there was less polarization, and conservative interests were generally better served during these periods in Brazil and Chile.

Most cases do not, of course, fall at the xtreme values of either dimension. For the umerous intermediate cases, however, this liscussion of patterns nonetheless points to ome of the underlying issues that lead counries to position themselves at different points long these dimensions.

In summary, whether one considers patterns of change within countries or comparisons cross countries, one may reasonably argue that here is an important relationship between lifferent patterns of inducements and contraints and different political contexts. Whereas with a unitary concept of corporatism one ould only note changes in the overall level of orporatism, this disaggregated approach permits more differentiated observations and comparisons concerning what the government is loing and what is happening to organized labor.

Conceptualizing State-Society Relationships

We have suggested that recent discussions of orporatism have played a central role in the enewed effort to discover more adequate ways of conceptualizing alternative patterns of stateociety relations. How does a focus on inducements and constraints contribute to this larger ffort?

First, this focus has the advantage of being nteractive, of pointing to an implicit or explicit argain or transaction that is struck at a articular time, reflecting the existing constelation of power relationships and the goals of elevant actors. This idea of a bargain is not ntended to imply that the corporatized group, uch as labor, is always actively involved in a ormal process of bargaining. In many cases, abor is only a passive participant, and the legree to which labor is actively involved is ndeed one of the factors that affects the alance that is struck between inducements and onstraints.

Second, this interactive approach is dynamic n that it encourages the analyst to look for atterns of change over time. Once one has onceptualized state-group relations in terms of bargain or transaction that reflects the existng configuration of power relationships and olitical goals, it become obvious that in the context of changing power relationships and changing goals, the terms of the bargain may be renegotiated. This tendency is well illustrated by the volatile pattern of change in inducements and constraints in Argentina noted above.

Third, because the political role of organized labor has been treated as a central issue in recent efforts to develop broad typologies of national political systems in Latin America, the distinction between inducements and constraints can make a useful contribution to refining these typologies. For instance, authors such as O'Donnell (1973, 1978) argue that the repressive authoritarian governments which have recently emerged in the industrially more advanced countries of Latin America represent a new type of political system—referred to as "bureaucratic-authoritarian." These systems are seen as involving a complex constellation of characteristics, including the political and economic "exclusion" of organized labor, i.e., the exercise of strong control over both the organizations and the income of this sector.

More recent studies have suggested that while the concept of bureaucratic-authoritarianism has made a major contribution, a more adequate analysis can be achieved if this concept is disaggregated and important variations among its component elements are examined separately (D. Collier, 1978; Cardoso, 1979; and Kaufman, 1979). The distinction between inducements and constraints provides a useful starting point for carrying out a disaggregated analysis of a crucial feature of bureaucratic-authoritarianism: the approach adopted for controlling organized labor. This may be illustrated through a comparative discussion of four contemporary cases—Brazil, Mexico, Chile, and Uruguay—which have been identified as bureaucratic-authoritarian.

In Brazil, the high-inducements/high-constraints pattern noted in Figure 2 persists. That is to say, the state exercises sharp control over labor organizations, in important measure through attempting to maintain organizationally viable unions that are co-opted by and dependent on the state—but that are of virtually no importance as coalition partners for the government. Though important periods of worker protest in both the late 1960s and late 1970s threatened this system of control, it has been the predominant approach in the post-1964 bureaucratic-authoritarian period (Erickson, 1977). Contemporary Mexico has likewise been characterized as bureaucratic-authoritarian (O'Donnell, 1978), yet the distinct pattern of inducements and constraints in Mexico noted in Figure 2 persists to the present day. Wage policy has been exclusionary—as reflected in the decline in the real income of workers after 1976. Yet in contrast to the Brazilian pattern, organized labor remains a crucial partner in the dominant coalition. In exchange for this support, major organizational inducements have been extended to labor leaders—for instance, in the post-1976 period—in order to secure cooperation with the wage policies.[13] Thus, the Mexican system has been characterized as involving "two carrots, then a stick" (Smith, 1979, p. 57). Finally, as we indicated in the discussion of Figure 1, in Chile and Uruguay one finds a still different pattern, involving a system of pure constraints. Here, the existing labor organizations played no role in the support coalition of bureaucratic-authoritarianism, and there was virtually no reliance—at least in the initial period of military rule—on an effort to maintain a system of organizationally viable, co-opted unions.

Different combinations of inducements and constraints are thus found in these countries, and the analysis of the relationship between these two dimensions helps to bring into focus important differences in the contemporary pattern of state-labor relations among these four cases. The analysis of differences such as these can play a useful role in achieving a more adequate description and dynamic analysis of the emergence and evolution of bureaucratic-authoritarianism.

A fourth issue concerns whether this distinction between inducements and constraints may be applied to the relationship between the state and other types of groups. In light of O'Donnell's (1977) important argument that corporatism is "segmental," in the sense that it means different things for different class groups, this issue merits attention here. Certain aspects of structuring and subsidy are, of course, especially salient to labor organizations, and one should not assume that the particular inducements considered in this article will be equally relevant to all types of groups. For instance, because of the economic position of labor unions in society and the organizational requirements for effectively engaging in strikes and collective bargaining, such provisions as subsidy, monopoly of representation, and compulsory membership may be far more relevant to unions than to groups such as business associations. However, though the particular inducements—and constraints—relevant to other types of associations may vary, it seems likely that at a more generic level, the perspective of viewing state-group relations in terms of an interplay between inducements and constraints will be relevant for other types of groups as well.

Fifth, though initially formulated with reference to Latin America, this focus on inducements and constraints can contribute to the broader comparative analysis of different patterns of state-society relationships. This broader application may be illustrated by examining the relationship between the inducements-constraints distinction and Schmitter's (1974, 1977) distinction between "state" corporatism and "societal" corporatism. He argues that the Latin American cases considered in this article, and more generally other cases of corporatism in the Third World and Southern Europe, involve *state* corporatism, in that the corporatized groups "are created by and kept as auxiliary and dependent organs of the state which founds its legitimacy and effective functioning on other bases" (Schmitter, 1974, pp. 102–03). Schmitter uses the expression *societal* corporatism, by contrast, to describe systems of post-pluralist interest representation in advanced industrial societies in which corporative patterns of state-group relations have emerged in contexts in which "the legitimacy and functioning of the state [are] primarily or exclusively dependent on the activity" of the corporatized groups. In the first case, interest associations are thus "dependent and penetrated"; in the second case, they are "autonomous and penetrative" (1974, pp. 102–03).[14]

The distinction between state and societal corporatism involves the same issues of power relationships and bases of political support that we have used as a basis for distinguishing *among* Latin American cases. Hence, while as a first approximation it seems appropriate to characterize state-labor relations in Latin America as involving state corporatism, it is useful to go beyond this assertion and treat the distinction between state and societal corporatism not as a dichotomy but as a continuum, with some of the Latin American cases located at least part-way along this continuum toward societal corporatism. While Brazil stands out as one of the clearest cases of state corporatism, the discussion of the interplay between control and support in Argentina and Mexico suggest that these cases, at least during certain periods, are nearer to the middle of a state-societal continuum.

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[13] See Stefanowicz, 1979. We acknowledge our debt to Susan Kaufman Purcell for calling our attention to the importance of these measures. It should be stressed that these particular inducements were not among those included in the scoring for Figure 1.

[14] Elsewhere Schmitter seems to point to a somewhat different definition. Whereas on pp. 102–03 he stresses these issues of dependency and autonomy, on pp. 103–04 he stresses the issue of whether corporative provisions are initiated from above or from below. The locus of initiative can vary independently from the pattern of dependency and autonomy. The question of the locus of initiative is obviously important, but within the framework of Schmitter's discussion it appears to be a subordinate issue. We therefore focus our analysis on the larger issue of autonomy and dependency.

As one moves beyond the variations within Latin America to contexts that involve more nearly pure cases of societal corporatism, one might expect greater emphasis on inducements and less on constraints, since these would be contexts in which the state was more dependent upon the corporatized groups. These are commonly situations in which the state ratifies patterns of non-competitive interest representation that emerged "from below" involving strong, autonomous interest groups. Within the European context, a major inducement that has appeared in such cases has been the opportunity for certain groups to be represented on functionally organized, semi-public entities such as wage-price councils and economic planning boards.[15] This contrast between the patterns of inducements and constraints that one might expect in hypothetically "pure" cases of state and societal corporatism is reflected in the right side of Figure 3.

The left side of Figure 3 suggests the hypothetical relationship between the other two combinations of "extreme" values of inducements and constraints and other types of group representation. High levels of constraints and low levels of inducements by definition involve a situation of outright control or repression of groups—cases of which have already been noted above. Low levels of inducements and constraints involve situations in which the state does not attempt to shape

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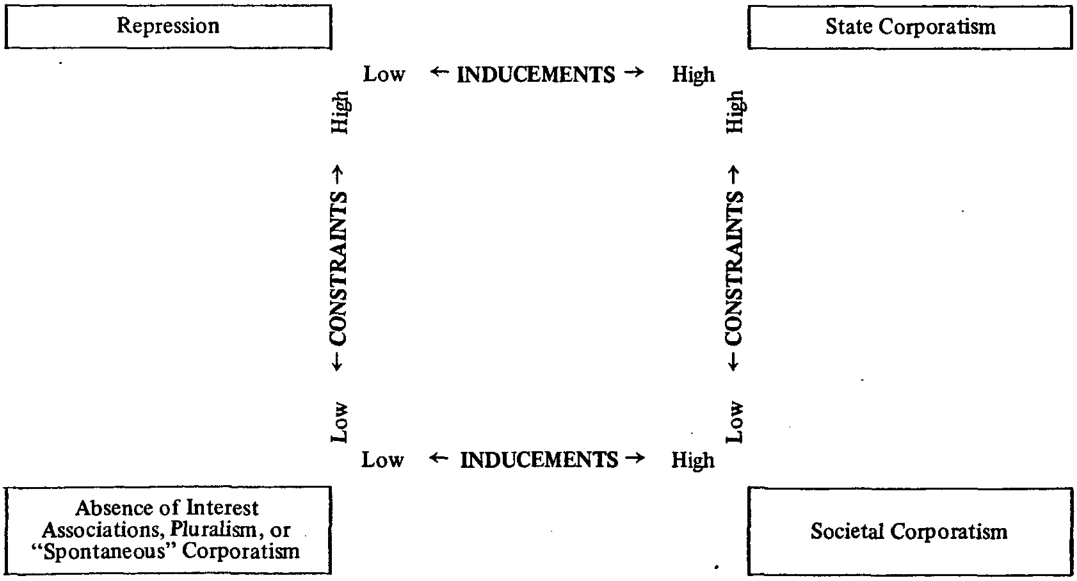
[15] Schmitter excluded these entities from his definition of corporatism because he observed that they did not consistently occur in conjunction with the set of characteristics that he defined as corporatist (1974, p. 94, n. 24). We have likewise excluded them in the operationalization presented above, partly for the same reason and partly because of the extreme difficulty of establishing a reliable scoring procedure for such entities. For a discussion of their importance within the Latin American setting, see O'Donnell, 1977.



Source: Compiled by the authors.

Figure 3. Hypothetical Relationship between Inducements-Constraints Distinction and Broad Types of Group Representation

interest politics through these kinds of provisions aimed at interest associations. This pattern may correspond to situations in which interest associations do not exist, to situations of pluralism, or to situations in which pluralism may have been eroded "from below" through the oligarchic tendencies of group interaction but in which the state has not become involved in ratifying or reinforcing this erosion of pluralism. This is a substantial "residual category," which reflects the obvious fact that a focus on inducements and constraints does not allow one to distinguish among all different patterns of group representation. This focus does, however, provide a useful perspective for looking at variations in the state role in group representation.

In addition to stressing the utility of this distinction between inducements and constraints, we should note the limitations of this perspective as it has been presented here. First, the particular operationalization of inducements and constraints we have proposed is not intended to encompass all of the inducements and constraints formalized in labor law—not to mention those found in other areas of law or those not embodied in law at all. The purpose of this operationalization is to show that there are certain recurring patterns of inducements and constraints. The particular scoring of inducements and constraints presented above is not intended to be a definitive assessment of these two dimensions.

Second, the analysis has focused on two crucial actors—the state and labor organizations. Obviously, other actors are closely involved in the interplay of inducements and constraints that we have analyzed—most immediately the workers themselves (as opposed to labor *organizations*) and employers. What is ultimately called for is a far more complex analysis that encompasses, at the very least, all four of these actors. Thus, in one context, the state may extend important benefits to labor organizations to strengthen the position of these organizations and of workers vis-à-vis employers. In another context, both the state and employers may extend inducements to labor organizations in order to secure their aid in enforcing regressive income policies on the workers. At this level, highly complex relationships may be involved. The goal of the present analysis has simply been to propose a conceptual distinction that will make it easier to analyze these more complex patterns.

Finally, this attempt to conceptualize more adequately different patterns of state-society relations is a building-block in a larger analytic effort in another sense as well. A more sharply focused *description* of these relationships should ultimately contribute to a more adequate *explanation* of differences among countries and change over time within countries. It should help to address questions such as: Why is the pattern of state-labor relations that emerges at the time of the initial "incorporation" of organized labor sustained over many years in some countries, whereas in others it is not? Why have such different systems of labor control recently emerged in the context of "bureaucratic-authoritarianism," and what are the implications of these different systems of control for the ability of labor to achieve a more favorable distribution of political power and economic resources?

The answers to these questions can help us to understand certain anomalies in the long-term patterns of change followed by different countries. For instance, Figure 2 pointed to similarities in patterns of inducements and constraints at an earlier point in this century between Argentina and Mexico, on the one hand, and between Brazil and Chile, on the other. Yet in the more recent period, if one examines the degree to which different countries have well-institutionalized systems of labor control, there appears to be a significant regrouping of cases. It might be argued that Mexico and Brazil now have more institutionalized systems of control, whereas Argentina and Chile have less well-institutionalized systems of control. How does one explain this shift? What are the "transformation rules" that account for these changing patterns? The analysis of inducements and constraints will have proved useful if it can help to answer questions such as these.

Appendix A.
Overview of Legal Provisions for Inducements and Constraints

Inducements

Registration. The first inducement, both in terms of the timing of its appearance in each country and in terms of the low level of "corporatism" it represents by itself, is the registration or official sanctioning of unions by the state. This has appeared in every country in Latin America. Registration confers specified rights, including typically juridical personality and the right to represent workers' interests before the employer and before the state. Prior to the appearance of registration in each country, unions had either been repressed or could become legally incorporated only under general legislation concerned with freedom of association. In some important cases this mode of incorporation was not an attractive alternative to unions because the unlimited civil liability it imposed appeared undesirable in contexts in which unions might be held responsible for damages incurred during a strike (I.L.O., 1930, p. 163). The provisions for official registration considered here created a separate type of incorporation that was particularly suited to the needs of unions. The point at which registration first appears is, in most cases, the point at which labor law first emerges as a distinct body of legislation designed to encourage the formation of worker organizations.[16]

Right of Combination.[17] These provisions facilitate the formation of unions, primarily by protecting them from various forms of harassment by employers. Because they protect the right of unions to exist as organizations, these provisions might be seen as providing a basis for either corporatism or pluralism. However, the notion that the state has to intervene actively in society in order to make it possible for workers' organizations to exist already takes one beyond the vision of interest politics contained in the conventional version of pluralism and into the sphere of state involvement in shaping interest politics that is the focus of our analysis.

Monopoly of Representation. This involves the issue of the degree to which there is an absence of competition among unions to represent workers in a particular occupational grouping. The relevant provisions range from the few cases in which several unions within a given occupational grouping are allowed to compete for members and to bargain with employers to the exclusive granting of the right of representation to a single union.

Compulsory Membership. This rarely exists in the form of an outright legal requirement that all workers must become union members. Rather, one finds a series of partial approximations. Some countries have legal provisions that permit collective bargaining agreements to include clauses requiring all workers to be union members. In other cases there are requirements that nonmembers be subject to the same authority or obligations as members with regard to specific issues. For instance, collective bargaining agreements may apply to nonmembers as well as members, or nonmembers may be subject to a dues check-off.

Subsidy. Worker organizations are of course subsidized in a great variety of ways, both formal and informal. The provisions of interest here are those which involve the state directly in helping unions receive a regular source of revenue.

Constraints

Collective Bargaining and Strikes. Collective bargaining over wages and working conditions is one of the most important areas of activity and demand-making of labor organizations, and the strike is labor's most important weapon. One of the most significant types of state control involves intervention in collective bargaining to avoid class conflict and the disruption of economic activity. In recent years the state has become involved in setting wages and in decreeing other policies that have removed much of the substance of collective bargaining to the area of administrative and/or judicial decision. This state role has become so complex that it was not feasible to score all aspects of it, particularly the state role in wage-setting. However, the provisions considered in the present analysis, which regulate the conditions of collective bargaining and the use of the strike, represent a large proportion of the important regulations in this area, particularly for analyzing the earlier phases in the emergence of labor law in Latin America.

Other Constraints. A series of additional constraints are commonly found which (a) limit the kinds of demands unions may make and the kinds of demand-making activities in which they may engage; (b) control leadership and leadership recruitment with the goal of restricting the role of radical political groups, or earlier in this century, radical foreign immigrants; and (c) give legal sanction to direct state monitoring and intervention in internal union affairs.

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[16] In light of the importance of registration, it might have been given more than a single point in constructing the scales (see Table 1). However, because all countries adopted it at a relatively early point in the evolution of their labor legislation and because no countries rescinded it, this somewhat arbitrary choice to give it only one point had little effect on the analysis presented below.

[17] For a discussion of the "right of combination" of workers, as opposed to the "freedom of association" of groups in general, see Bendix (1964, pp. 80–87).

Appendix B.
Coding and Scaling of Labor Law

Table 1 presents an outline of the categories used in the actual coding. The first step in scaling the data generated by this coding involved constructing "sub-scales" that corresponded to the nine headings in Table 1. Considering first the five subscales concerned with registration, monopoly of representation, compulsory membership, subsidy, and control of collective bargaining and strikes, one can see that there is an inherent ordering among the categories under each of these headings and that at any given time, each country can logically be in only one of the categories under each heading. Hence, as they stand, the categories under each of these five headings repre-

Table 1. Outline of Categories Used in Scoring Labor Law

Inducements

1. *Registration.* Countries were scored zero or one according to whether there was a provision for the registration of labor unions.
2. *Right of Combination.* Countries were scored zero or one according to whether they had each of the following provisions:
   a. Employers may not make membership in a union an obstacle for obtaining or retaining employment.
   b. Employers cannot refuse to participate in collective bargaining with a legitimate representative of the workers.
   c. Unions may form into federations and confederations.
   d. Employers may not be organizationally involved in unions.
   e. Union leaders have some form of job security.
3. *Monopoly of Representation.* An ordered scale was constructed on the basis of the following values:

0 = No provision.

1 = Either multiple, competing unions are allowed to register and to represent members and bargain collectively, or there is the single restriction that for the purpose of collective bargaining, competing unions must form an inter-association committee.

2 = Competing trade or works unions may exist, but only the largest union among those representing the same sector may enter into collective agreements.

3 = Within a given sector, only singular unions are permitted.

4 = Prohibition of all unions except for one works union per enterprise.

4. *Compulsory Membership.* An ordered scale was constructed on the basis of the following values:

0 = No provisions.

1 = *Either* dues collection (or its equivalent) *or* collective bargaining agreements apply to both members and non-members.

2 = *Both* dues collection (or its equivalent) *and* collective bargaining apply to members and non-members.

3 = Closed shop or union shop clauses are permitted in collective bargaining agreements.

3.5 = Once a union is legally formed, membership is compulsory.

4 = Legal requirement of universal membership.

5. *Subsidy.* An ordered scale was constructed on the basis of the following values:

0 = No provision.

1 = Dues check-off permitted if union requests it.

2 = Dues check-off combined with closed shop provision or compulsory membership, or dues check-off that applies to members and non-members.

2.5 = Some form of on-going, state involvement in the financing of unions, such as the provision in Chile for a form of profit-sharing in which a portion of the profits of the enterprise are paid to the union via a state agency.

3 = Syndical tax.

(continued on next page)

Table 1 (continued)

Constraints

1. *Collective Bargaining and Strikes.* An ordered scale was constructed on the basis of the following values.

   0 = No provision.
   1 = Voluntary conciliation and/or arbitration preceding a strike.
   2 = Compulsory conciliation preceding a strike.
   4 = Compulsory arbitration to be initiated at a specified time after a strike begins.
   5 = Compulsory arbitration before a strike.
   7 = Strikes outlawed.

   Because there are often special conciliation, arbitration, and strike provisions that apply to the public sector, to public services, or to other sectors viewed as crucial to the economy, additional points were added to this score as follows: one point if strikes were outlawed in the public sector; one point if strikes were outlawed in public services or other "strategic" sectors; half a point if the score for the public sector on the above scale was higher than the score for industrial disputes in general (unless it was equal to seven, in which case it got a whole point); and half a point if the score for public services or other strategic sectors on the above scale was higher than the score for disputes in general (again, unless it was seven). The highest possible score was thus nine.

2. *Other Constraints on Demand-Making.* Countries were scored zero or one according to whether they had each of the following provisions:

   a. Collective bargaining agreements must be approved in order to be legal.
   b. Political activities of unions prohibited.
   c. Political and/or solidarity strikes prohibited.
   d. Boycotts and/or picketing prohibited.
   e. Calling an illegal strike constitutes grounds for dissolving a union.
   f. Union officials can be removed for calling an illegal strike.

3. *Leadership.* Countries were scored zero or one according to whether they had each of the following provisions:

   a. Salaries of union officials regulated.
   b. Union officers must be workers in the occupational grouping which their union represents.
   c. Union leaders must meet citizenship and/or residence requirements.
   d. Political activists associated with certain political parties or ideologies are excluded from union office.

4. *Internal Governance.* Countries were scored zero or one according to whether they had each of the following provisions:

   a. The state can audit union financial records.
   b. Expenditures of union funds regulated.
   c. State official may attend union meetings.
   d. State authorized to assume direct control of unions (to "intervene" them).
   e. State may dissolve unions. For this crucial provision a score of zero was assigned if there was no provision; a score of one if it could be done by judicial decision only; and a score of two if it could be done through a more discretionary administrative or combined administrative and judicial decision. A further point was added if the permissible causes for dissolution went beyond narrow procedural criteria to include broader political criteria. The maximum possible score for this trait was thus three.

sent unidimensional scales.[18]

The provisions under the remaining four headings—right of combination, other constraints on demand-making, leadership, and internal governance—are not inherently mutually exclusive. Rather, it is logically possible for any one country to have several of the provisions simultaneously. Guttman scaling was therefore used to discover if cumulative, unidimensional scales were present. The provisions did in fact follow a Guttman scale pattern,[19] and for each of the scales each nation was given a score according to the number of provisions in the scale that it possessed.

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[18] For three of these five scales (see Table 1) we followed the practice recommended by Tufte (1969, p. 646) of assigning slightly different intervals on the basis of a substantive interpretation of the relative "distance" between the different provisions. If slightly different choices were made regarding these weightings, or if no weightings were used, it did not significantly change the results of the scaling analysis.

The relationships among these nine subscales were then analyzed to determine the degree to which it was statistically appropriate to form an overall index of inducements and of constraints. On the basis of cluster and factor analysis we concluded that there were strong associations both among the five subscales under the inducement heading and among the four subscales under the constraint heading, and that it was therefore appropriate to group the scales in this way.[20] Aggregate measures of inducements and constraints were then derived on the basis of these analyses.[21] It should be noted that the composite scales of inducements and constraints are themselves strongly correlated (.84). Hence, it would also be meaningful to form an additional composite scale that combines all nine subscales which could be referred to as an overall measure of "corporatism." In any particular analysis, whether one analyzes the two dimensions separately or together thus depends on the theoretical concerns of the study. In the present article, our goal is to discover whether interesting patterns emerge if these two logically distinct dimensions are treated separately, and therefore we have employed them as separate indices.

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[19] Guttman scale analysis was selected as a technique for aggregating the component elements of these subscales because of an important property of these elements that was quickly evident from an inspection of the data. It was clear that while some of the elements were "easier," in that they appeared earlier and in more countries, others were "harder," in that they appeared later and in fewer countries. This resulted in a "non-linear" pattern of relationships among the elements that made statistics such as product-moment correlations or tau less appropriate. The question remained, however, as to how regular this ordering from "easier" to "harder" in fact was. This is precisely the question addressed by Guttman scale analysis. The elements involved in the four subscales under consideration here do, in fact, follow a fairly regular Guttman scale pattern. The coefficients of reproducibility are in all cases above .92 and the coefficients of Guttman scalability are above .8 for two of the scales, .72 for the third, and .63 for the fourth.

[20] The cluster analysis was based on the coefficient gamma, which was deemed appropriate because of the "nonlinear" property of the relationships referred to above in the discussion of Guttman scale analysis. Among the inducements subscales the mean value of gamma was .82, with the lowest coefficient being .74. Among the constraints subscales the mean value was .80, with the lowest coefficient being .69. Though the data do not meet all of the assumptions of factor analysis, it is useful to note as well the findings derived from this technique because the percent of variance explained by the first general factor provides a convenient summary of the degree to which a set of items go together. For the inducements subscales, the first general factor (using the principal factor method with iteration) explained 73 percent of the variance, with the lowest loading being .74. For the constraints subscales, the first general factor explained 82 percent of the variance, with the lowest loading being .81.

[21] Cases were assigned overall scores for inducements and constraints by summing the appropriate subscales. Though, as noted above, the data do not fully meet the assumptions of factor analysis, it is again useful to note the results that would have been provided by this alternative method, since they suggest that the findings reported here are not narrowly dependent on any one technique. The general factors that were extracted (see above) both had correlations of over .96 with the combined scales that were formed by summing the subscales.

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Criticism, Cynicism, and Political Evaluation: A Venezuelan Example

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Political trust and political efficacy are concepts currently undergoing considerable discussion and revision. The discussion of voting behavior in Campbell et al. (1954), Almond's and Verba's (1965) version of the participation hypothesis, Gamson's (1968) trust-efficacy hypothesis, and Verba's and Nie's (1972) standard socioeconomic model represent quite distinct and contrasting stages in the treatment of these concepts, but share some assumptions requiring additional discussion and clarification. One of these pertains to treatment of political criticism, *measured with low scores of trust in government, as political* cynicism. *This is not so much an operational or methodological question as a theoretical framework that has never had much to say about the role of criticism in contemporary democratic societies. This essay proffers a case in which, high levels of interpersonal distrust and high levels of government criticism notwithstanding, it is possible to make this basic distinction. Path analyses of data from a national-sample survey show that criticism is triggered by predominantly political orientations, while cynicism is a rigidified form of criticism inseparable from the social circumstances of individuals. In addition, the evidence suggests that the sense of political efficacy does not play the pivotal role assigned to it in the literature.*

Discussions of political criticism seldom take nto account the *context* in which individuals elate to the political system. Instead, the paradigm dominating the literature until recently (Almond and Verba, 1965; Campbell et al., 954) assumed that political criticism is part of cluster of attitudes characterized by political pathy, low "trust in government," and a low "sense of political efficacy" (see Balch, 1974, p. 2 on Campbell et al., 1960; Verba and Nie, 972). Criticism of political institutions was believed to originate in a marginal socioeconomic status which obscures or prevents clear understanding of the workings of the political ystem, or in a very atypical pattern of socialization which, coupled with somewhat exotic ideological orientations, considerably distort an ndividual's perception of the system. Therefore, criticism has been treated as a symptom of ndividual frustration, and not as a result of hortcomings in the performance of the political system which may be perceived by individuals. Whether conceived in terms of the "participation hypothesis" (Almond and Verba, 1965; Verba and Nie, 1972), or of the "trust-efficacy ypothesis" (Gamson, 1968; Fraser, 1970; Seligson, 1977), the tendency has been to treat political criticism as uninformed (Converse, 1964) or subversive (Lane, 1962).[1]

Recent efforts to evolve more sophisticated measures of "sense of political efficacy" (Seligson, 1978), to evaluate the validity and reliability of the traditional measures of the sense of efficacy (Asher, 1974; Balch, 1974; McPherson et al., 1977), and to discuss the conceptual and practical implications of the decline of trust and efficacy scores in the United States (Miller, 1974; Citrin, 1974; and Niemi, 1975) suggest that a revision of the dominant paradigm is afoot. However, most of these contributions are concerned with attitudes assumed to accompany criticism, and not with criticism itself.

This essay is an attempt to introduce a new element into the ongoing revision, namely, the distinction between *criticism* (from Webster's: "judging with knowledge and propriety") and *cynicism* (again from Webster's: faultfinding, captious criticism). In trying to establish this distinction, I will treat both as aspects of political evaluation, albeit with different causal origins. More specifically, I interpret the evidence to be presented and discussed as showing that cynicism originates in the social circumstances of individuals, while criticism has explicitly political origin. The ability to make this distinction would, or perhaps should, have

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[1] Even Lane, who thought that ordinary citizens may come to the conclusion that the government is untrustworthy, treats political criticism as political alienation (1962, p. 473).

theoretical relevance in evaluating "the interaction between citizen and government . . . that democratic institutions become in practice" (Converse, 1975, p. 87). More importantly, the distinction should enable us to take political criticism out of the subversive context to which it has been relegated, and to facilitate interpretation of the functions of criticism in contemporary democratic societies (Inglehart, 1977, pp. 303–07).

In choosing the case of Venezuela to illustrate the distinction, I am not simply making opportunistic use of data collected there, in a national sample survey of 1,521 individuals interviewed in the fall of 1973.[2] To be sure, in using Venezuela I have a little more latitude to try to add a new outside element to a discussion taking place among students of public opinion in the United States. I can put some distance between this presentation and that discussion, pretending that I am dealing with a different case. However, the substantive issues remain the same, the items that measure criticism and cynicism for the Venezuelan case are essentially the same traditional measures of political trust and sense of efficacy found in the literature,[3] and the high levels of public criticism found in Venezuela demand a careful methodological treatment.

Context and Case: Political Criticism in Venezuela

Venezuela boasts a democratic regime anchored by a multiparty system dominated by the Social Christian (COPEI) and the Social Democratic (AD) parties. In the last three elections of 1968, 1973, and 1978, the incumbent party has lost the presidential race and power has been transferred without incident. The Left is not persecuted by "death squads" operating under official protection; instead, the Left is represented in Congress by three different parties which seek to transform "the establishment" by constitutional means. The reality of turnover is not lost on the electorate. Support for the institution of elections is very, very high and since 1958, when the present democratic regime was inaugurated, voting turnout has hovered around 90 percent (Martz and Baloyra, 1976, pp. 46–47, 281).

In Venezuela, democratic governments have done a better job than military regimes in maximizing fiscal revenues and allocating these resources to the development of the country's human and natural resources (Baloyra, 1974; Hanson, 1977; Kuczynski, 1977). However, there is great discontent with the performance of the managers of the regime. Many citizens find fault with the regime's policies, with the role of parties and politicians in the political process, and with the performance of the bureaucracy, although this criticism falls considerably short of generating support for a return of the military to power (Martz and Baloyra, 1976, 1976, pp. 48–51).

The fact that the Venezuelan democratic regime is able to function in a context of substantial public criticism suggests that such criticism is not so destructive, for if a relatively fragile and young regime can cope with critical opposition, then there is reason to believe that more institutionalized regimes can cope with highly critical, mobilized electorates. Therefore, as Citrin (1974) has suggested, it is not just the incidence of criticism—or cynicism, as he calls it—but, in addition, that organizational alternatives are available to critics. In Venezuela there are such alternatives.

However, it could be argued that the high levels of political criticism found in Venezuela, which is after all one of the more democratic and open societies of Latin America, are the result of cultural nuances and individual psychological states. For example, levels of interpersonal trust are very low in Venezuela. Most respondents in the 1973 sample believed that one should "peel the eye" instead of trusting others, that people mind their own business and are not helpful, and that if one is careless, others take advantage.[4] Scaling these responses with Guttman criteria, one finds that

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[2] For a description of the research design of the study and the characteristics of the sample utilized, see Baloyra and Martz (1979, pp. 195–206).

[3] There is no question that extant measures of trust and efficacy could use some improvement. I am aware that Converse suspects the dimensionality of the scale of sense of political efficacy since the "voting is the only way" item no longer scales with the other three (1972). For the Venezuelan data to be utilized here, the four-item battery of efficacy items did not scale. Following Balch (1974, pp. 22–26) the items in "pair B," "government officials don't care" and "people like you have no say" were combined with two items dealing with the perceived influence (present and in the near future) of the respondent's reference group (people like yourself). Therefore, even though I use traditional items purported to measure the sense of political efficacy, I am sensitive to the problems discussed in the literature concerning the validity and reliability of the items.

[4] The percentage of respondents proffering these opinions are 91, 82 and 84 percent, respectively.

75 percent of the public belongs in the highest category of what has to be treated as a measure of interpersonal distrust. In addition, Venezuelan political culture has been described as imbued with a generalized sense of distrust and suspicion which lend it a paranoid character (Silva Michelena, 1970, p. 240).

These characteristics of the Venezuelan political context could, according to this alternative interpretation, explain why 56 percent of the public are very critical or critical of the policies of the democratic regime, why 49 percent are very critical of party politicians, and why 52 percent gave very low marks to the incumbent Christian Democratic administration.[5] The reasoning here would be that the high levels of criticism found in Venezuela are simply a reflection of generalized individual distrust which predisposes the Venezuelan public to adopt a very cynical perspective toward the political process. Therefore, cultural and psychological factors are the main determinants of the high levels of dissatisfaction found in Venezuela. By contrast, the interpretation followed in this article is that one should differentiate between cynicism and criticism in order to make such an appraisal, and that if one is to find any guidance in the literature concerning this distinction, one should attribute greater importance to the impact of cultural and psychological factors on cynicism, since political criticism is the result of predominantly political factors.

Criticism and Cynicism in Venezuela

One way to make a distinction between cynics and critics is by their responses to statements containing sweeping generalizations. One can phrase items in a manner in which both the item and the individual are on the underlying attitudinal continuum. By phrasing items in a very extreme fashion one overspills the underlying attitudes and there is no one-to-one correspondence between response categories and the attitudinal continuum (Torgerson, 1957, p. 305). Other items measuring similar attitudes with less extreme language yield categories which can be ordered, assigning their boundaries to positions on the underlying continuum; by assigning the subjects to the ordered combinations of these boundaries one builds a cumulative, unidimensional Guttman scale (Torgerson, 1957, pp. 302–04). One could identify critics as those respondents in intermediate Guttman-scale categories of measures of political evaluation and cynics as those who are in the more negative categories of such measures. In addition, cynics would be more likely to agree with extreme, maximum-probability items which cannot be fitted into Guttman scales.

Political criticism is measured with four Guttman scales. The first deals with criticism of the incumbent administration and includes opinion about the impact of administration policies on (1) "people like yourself," and (2) on the respondent's immediate family; (3) the sympathy felt by the respondent toward president Rafael Caldera; and (4) opinion about whether the administration of a different party could do a better job solving the nation's problems. A second Guttman scale measures criticism of the policies of the democratic regime from the standpoint of (1) whether the democratic governments of the last 15 years (1958–1973) have been beneficial to Venezuela, (2) whether they have spent public monies judiciously or have wasted them, (3) whether they have served the public interest or the interests of very powerful groups, and (4) whether they have been staffed with honest people or crooks. Criticism of the role of politicians is treated with a Guttman scale incorporating opinion about whether politicians (1) "worry about the problems of people like yourself," (2) "strive to solve the country's problems," (3) "perform a job of great benefit to the community," and (4) "talk much and do nothing." Finally, criticism of the institution of elections is measured with a Guttman scale which posits whether (1) "voting is a very important factor in politics," (2) "elections are necessary in order to have a democracy," (3) "elections force the government to worry about people's problems," and (4) "elected officials worry about the problems of the people who elected them." These four scales representing four different *modes* of political criticism will also be considered globally, combined into a composite index of political criticism. The distribution of respondents along the categories of the four scales of political criticism are reproduced in Table 1, below, together with the coefficients of reproducibility and scalability of each of the scales.

Political cynicism is measured with items gathering more extreme reactions to the political process. Although related to the continua of criticism, these items can and must be dealt with separately. For the data at hand none of the following items could be scaled with Guttman criteria: opinion about how frequently government officials make appropriate decisions, opinion about the capacity of government officials (bureaucratic competence), opinion about how much attention government officials pay to ordinary citizens (bureaucratic indolence), the belief that government would be better off without (the interference of) politicians, and the belief that parties only care about (winning) elections. These statements invite agreement with very cynical propositions, even in the very critical context of Venezuela, and seem to point to the conclusion that politics is a big waste of time. They will be treated as a composite index of cynicism, derived from a linear combination of the standardized scores of the five measures. Inter-item and scale-item correlations for the standardized scale of political cynicism are presented in matrix form in Table 2, below.

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[5] For additional information concerning levels of dissatisfaction about different aspects of the political process in Venezuela, see Baloyra and Martz (1979, pp. 52–55).

Efficacy and Criticism

Most versions of the participation hypothesis treat the sense of political efficacy as a result of socialization experiences, including those reflected on the social circumstances of the individual and those stemming from contacts with public officials. The path diagram presented in Figure 1 deals with this aspect of the participation hypothesis. Sense of political efficacy appears to be determined, primarily, by the social circumstances of individuals and by one mode of political criticism—namely, criticism of government policies. The model suggests that upper-status individuals feel more efficacious, subject to some deflation as a result of aging. On the other hand, higher levels of criticism found among lower-strata individuals seem to decrease their sense of efficacy even further.

Table 1. Guttman Scale Measures of Political Criticism

		Adjusted Frequencies				Coefficients of:	
Scales of	Missing Cases	Very Low	Low	Moderate	High	Reproducibility	Scalability
Criticism of the Incumbent Administration	78	18.1	30.3	38.7	12.7	.91	.65
Criticism of the Policies of the Democratic Regime	299	23.6	20.0	27.0	29.4	.90	.67
Criticism of the Role of Politicians	149	25.5	20.5	19.5	34.5	.89	.70
Criticism of the Institution of Elections	142	43.7	29.7	18.6	6.2	.94	.73

Source: Computed from data collected by Baloyra and Martz (1979, pp. 195–206).

Table 2. Composite Index of Political Cynicism

	Incompetence	Indolence	Parties	Politicians	Scale-Item
Government (always) fails to do what is right (29.7)	.23	.21	.25	.14	.52
Bureaucratic incompetence (27.7)		.15	.22	.23	.63
Bureaucratic indolence (78.5)			.22	.22	.57
Parties only concerned with winning elections (70.9)				.24	.60
Government better off without politicians (60.8)					.56

Source: Computed from data collected by Baloyra and Martz (1979, pp. 195–206).

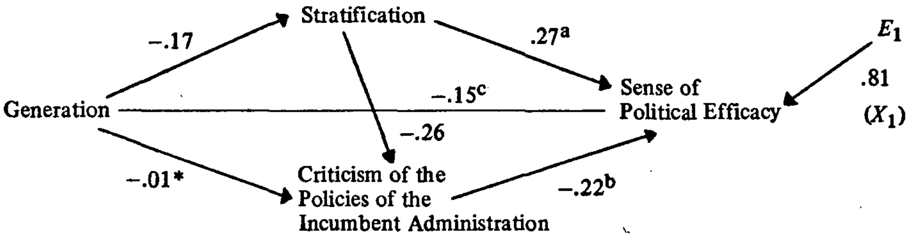
Note: Figures in parentheses represent the percentage of respondents expressing agreement with the most cynical option offered in the statements described in the text. All other figures are Pearson's correlation coefficients and are significant at the .01 level.

But, shouldn't the sense of efficacy determine criticism, instead of the other way around? In Venezuela, the sense of political efficacy depends on the individual's social circumstances and position in the life cycle; there is nothing unique about this. However, the sense of efficacy seems to depend also on the individual's evaluations of the policies of the incumbent administration independently of the former. Notice that the introduction of controls does not decrease the magnitude of the original correlation between sense of efficacy and criticism (–.26, at the bottom of Figure 1). Obviously, the sense of efficacy fluctuates with the level of criticism of the incumbent government regardless of one's age or social standing. Thus the degree to which individuals feel that they can influence the government is not a reflection of class ethos (optimistic/pessimistic) or generational perspective alone but also of one's evaluation of what the government is doing.

In the United States power is fragmented and individuals may acquire a sense of political efficacy with greater ease since there are more levels of government wielding effective power. In Venezuela, where power is more concentrated in the national executive, it may be more difficult to derive a sense of efficacy from contacts with public officials who do not have so much power after all. Whatever the real import of the differences in context between the two countries, it is clear that in Venezuela an individual's sense of efficacy springs from a baseline of class experiences and maturation, subject to fluctuations produced by the individual's evaluation of the performance of the government. More importantly, the sense of political efficacy appears as subject to change as any other attitude, although constrained by the factors described above.

Additional evidence suggests that it is more appropriate to treat the sense of efficacy as dependent on criticism rather than the other way around. Involved here is the question of whether the sense of efficacy increases one's level of satisfaction with the system and also individual allegiance to the system as suggested by Almond and Verba (1965, p. 191). These propositions are tested with the path diagrams presented in Figure 2.

It should be emphasized that, with the exception of the model involving criticism of administration policies, sense of political efficacy was included among the set of possible explanatory factors (or "dependent variables") used for *all* models. However, sense of political efficacy does not appear in *any* of the models presented in Figure 2, which inventories the more relevant sources of these different modes


Stratification
–.17
.27a
E_1
Generation
–.15c
Sense of Political Efficacy
.81
(X_1)
–.26
–.01*
Criticism of the Policies of the Incumbent Administration
–.22b

Source: Computed from data collected by Baloyra and Martz (1979, pp. 195–206).

Note: In this and subsequent figures I follow the rules of graphic representation for path analytic models, discussed by Kim and Kohout in Nie et al. (1975, pp. 389–92). All paths from exogenous (independent) variables to the dependent variable are standardized (beta) regression coefficients. All paths between exogenous variables are zero-order product-moment (Pearson's) correlation coefficients. Curved lines without arrows represent non-causal covariation. Disturbance (error) terms (E_s) are calculated for the lowest-order variable only. For simplification, nonsignificant and/or theoretically uninteresting relationships are not always represented.

[a] Originally .35
[b] Originally –.26
[c] Originally –.01*
*Not significant
N = 629

Figure 1. Sources of Sense of Political Efficacy in the Venezuelan Electorate

of political criticism. The first three models deal with the relationship between efficacy and evaluation of the performance of three different political actors: the incumbent administration, the regime, and the party politicians. The fourth model taps one aspect of system allegiance, measured by evaluation of the institution of elections.

The models do not completely rule out the possibility of a causal effect of the sense of efficacy on modes of political criticism. However, they suggest that sense of political efficacy is not a relevant causal agent of criticism. On the other hand, the impact of one or another mode of political criticism on the sense of efficacy should not be exaggerated: only one of the four aspects of criticism examined in Figure 2 could be included in the model presented in Figure 1. Once the impact of the three causal factors included therein is accounted for, other modes of political criticism are trivial or irrelevant.[6] One other related point should not be overlooked: given the difficulties encountered by those trying to measure the reality bases of

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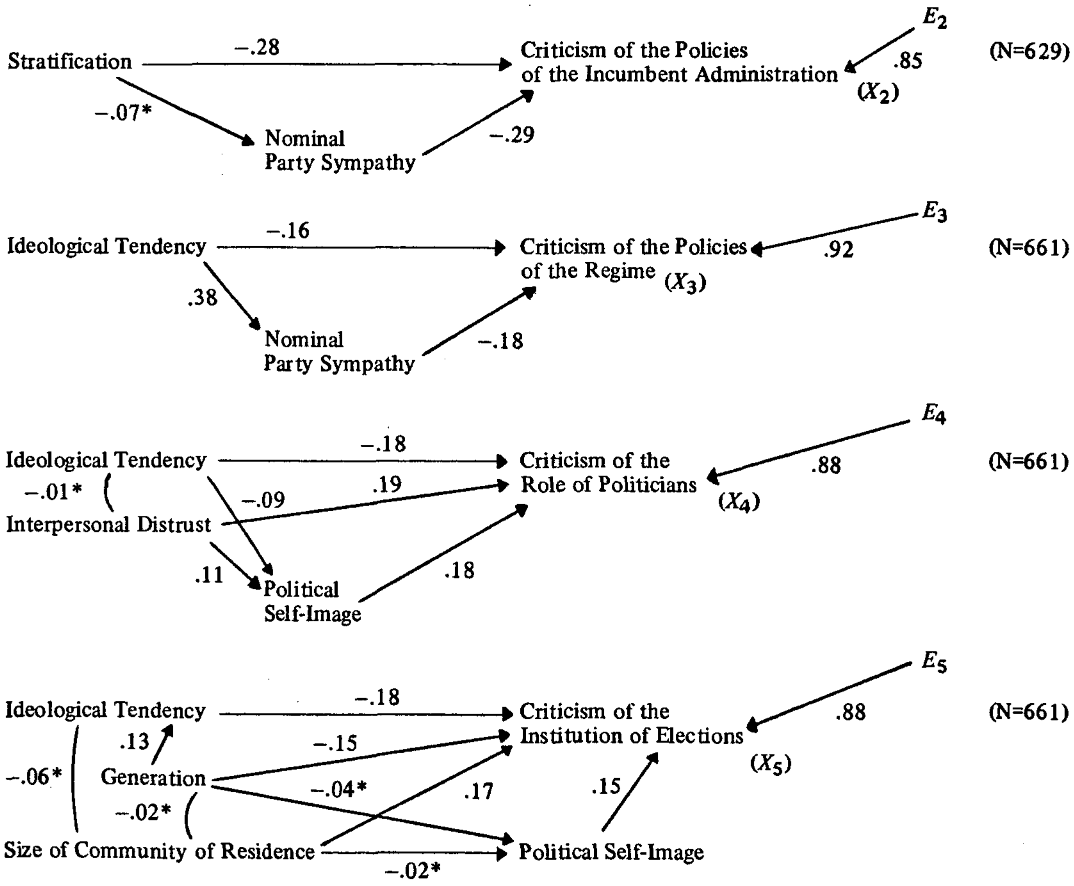
[6] The zero-order correlations between those modes of criticism and efficacy are not very robust. Expressed as Pearson's *r*s they are: –.07 for regime criticism, –.01 for criticism of elections, and –.14 for criticism of politicians, respectively. I am aware of Paige's (1971) contention that traditional measures of efficacy are "contaminated" by the individual's perception of government performance. This is precisely the point; the relationship exists where it should and, apparently, nowhere else. In the Venezuelan case it is an entirely reasonable assumption for a member of an opposition party to make that it will not be very easy to influence government policy. Looking at the Mexican case, Coleman and Davis conclude that it is evaluation of regime performance, and not socioeconomic status, that determines one's sense of efficacy (1976, p. 203).



E_2
Stratification
–.28
Criticism of the Policies of the Incumbent Administration
.85
(X_2)
(N=629)
–.07*
Nominal Party Sympathy
–.29
E_3
Ideological Tendency
–.16
Criticism of the Policies of the Regime
(X_3)
.92
(N=661)
.38
Nominal Party Sympathy
–.18
E_4
Ideological Tendency
–.18
Criticism of the Role of Politicians
.88
(X_4)
(N=661)
–.01*
.19
–.09
Interpersonal Distrust
.11
Political Self-Image
.18
E_5
Ideological Tendency
–.18
Criticism of the Institution of Elections
.88
(X_5)
(N=661)
.13
–.15
–.06*
Generation
–.04*
.17
.15
–.02*
Size of Community of Residence
Political Self-Image
–.02*

Source: Computed from data collected by Baloyra and Martz (1979, pp. 195–206).

Figure 2. Modes of Political Criticism in the Venezuelan Electorate

the sense of political efficacy (Weissberg, 1975), one could attempt to measure them indirectly, by looking at the separate causal effect of different modes of political criticism on an individual's sense of efficacy. Obviously, this would require an attempt to break down that *overall* sense of efficacy into its more specific components.[7] Then one could try to relate criticism and the sense of efficacy to the same kinds of political objects and, by implication, to the mode of participation which could serve as an empirical base for the individual's attempt to influence, and his/her subsequent evaluation of its success.

Further inspection of the path models presented in Figure 2 suggest that *political factors predominate* among the causal antecedents of the four modes of political criticism. Party preference, political self-image, and ideological tendency appear to be relatively constant influences. On the other hand, an individual's social circumstances do not appear to have a generalized impact since they are detectable in only one of the models, linking social stratification to criticism of the incumbent administration. The impact of this socioeconomic factor is relevant, but limited to one mode of criticism. Among the cultural factors which could predispose Venezuelans to display critical attitudes about politics and the political process, *interpersonal distrust* appears in only one of the models, while the impact of community of residence is restricted to criticism of the institution of elections. Finally, if one keeps an eye on the signs of the paths and the specific coding categories of the exogenous variables of the different models, one observes that the identity of the critics remains relatively constant across the different modes. Apolitical persons, independents, persons of leftist ideological tendencies, and sympathizers with leftist parties are more critical regardless of the object being evaluated.

Given the modal right-of-center orientation of the Venezuelan electorate (Baloyra and Martz, 1979, pp. 110–11) and the fact that Venezuelan politics can be described as a party system (Levine, 1973, p. 8), it is hard to imagine how the system could change in order to accommodate leftist and apolitical critics. On the other hand, they do not seem to be critical of the same kinds of objects since an individual's overall political orientation (political self-image) appears to be relevant in connection with politicians and elections, while the ideological motive extends to the outcomes of the regime as well. Somewhat more disturbing is that those who are more critical of the institution of elections include the young and the urbanites. One has to speak of erosion here since Venezuela is a predominantly young and urban country and becoming more so. Therefore, this may become a much more serious problem.

Even though we have emphasized disaggregating criticism into separate modes, it would be useful to repeat the analysis using a measure that will combine all the different modes. This would facilitate comparison with other findings reported in the literature, and would help validate and summarize the foregoing discussion. The model presented in Figure 3 uses a measure of criticism based on a linear combination of the standardized scores of the four modes of criticism discussed thus far. Interscale correlations are sufficiently low to pre-

Table 3. Correlations among Measures of Criticism

	Scale (X_6)	Regime	Elections	Politicians
Administration	.58	.32	.14	.26
Politicians	.70	.35	.41	–
Elections	.65	.29	–	–
Regime	.68	–	–	–

Source: Computed from data collected by Baloyra and Martz (1979, pp. 195–206).

Note: All values are Pearson's correlation coefficients, significant at the .01 level. Sample size is 661 for all computations.

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[7] One notable innovation popularized by Verba and Nie (1972) is the idea of participatory modes. Since the literature on "civic attitudes" attempts to relate these to participation, it is surprising that not much has been done to determine the dimensionality, that is the *modes* of these civic attitudes. Noteworthy exceptions are Balch (1974), Converse (1972), Coleman and Davis (1976), Muller (1970), and Seligson (1978).

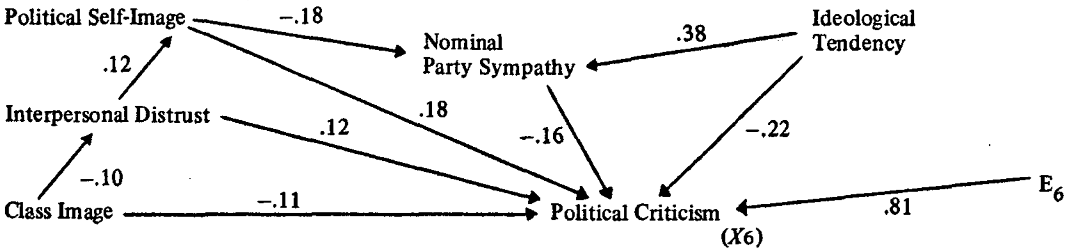
clude the assumption of cumulativeness among the four, as suggested by the data presented in Table 3.

Ideological tendency appears to have the strongest impact on this aggregate measure of criticism (X_6), followed by that of nominal party sympathy and of political self-image. When contrasted with the models presented before, this abbreviated model of political criticism affords no loss of information; once again we find voters of leftist ideological tendencies, sympathizers of leftist parties, and apolitical individuals to be more critical in an overall, aggregate sense. However, in spite of their limited impact which was confined to only one of the modes, both interpersonal distrust and social inequality are included in the model of Figure 3.

Two types of criticism appear implicit in the model. One is political and has little to do with social condition or interpersonal distrust. This is a type or mode of criticism following predominantly political considerations. This politicized criticism appears relatively independent of social or cultural influences and responds primarily to ideological and partisan considerations. It contrasts with the other type or mode, which originates in levels of interpersonal distrust found in the society as well as in social inequalities. I am tempted to call it social protest but the label may be a bit premature. However, it does not appear to be an artifact of the style of analysis since it seems reasonable to assume that the less specific the object of criticism, the more salient factors like distrust and social condition are going to be. One practical implication of this finding is that measures of "diffuse support" for a regime may tend to exaggerate the importance of the social bases of political criticism.

Political Cynicism

The point of departure for a contrast between political criticism and political cynicism ought to treat the former as a necessary but insufficient condition for the latter: all cynics are critical but not all critics are cynical. In addition, cynics, or better yet, level of cynicism among the electorate, are beyond the constraining influence of ideological or partisan considerations, as suggested by the model pre-



Source: Computed from data collected by Baloyra and Martz (1979, pp. 195–206).
Note: All paths leading to political criticism are fourth-order partial betas.

Magnitudes of the original zero-order (Pearson's) correlation coefficients between political criticism and:

Class Image = −.12
Interpersonal Distrust = .17
Ideological Tendency = −.29
Political Self-Image = .25
Nominal Party Preference = −.29

Not Represented:

Distrust and Nominal Party Preference = −.09
Distrust and Ideological Tendency = −.01*
Class Image and Party Preference = .00
Class Image and Political Self-Image = −.04*
Class Image and Ideological Tendency = −.06*
Ideological Tendency and Political Self-Image = −.09

*Not significant
N=666

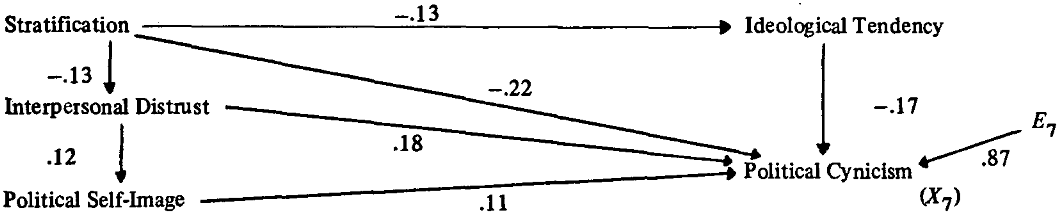
Figure 3. Sources of Political Criticism in the Venezuelan Electorate

sented in Figure 4, which combines the standardized scores of the five measures of cynicism (X_7) introduced earlier in the discussion. In this model, there is a bifurcation of causal paths somewhat similar to that discussed for criticism, but the partisan element is missing from the explanation. Stratification, interpersonal distrust, and ideological tendency appear more conspicuously with political self-image as the fourth exogenous variable.

This configuration identifies one basic source whose impact is independent of all others. Notice the lack of any difference in the magnitudes of the zero-order and partial betas between social stratification and cynicism. Obviously, the impact of stratification on cynicism is independent of any other source. A similar situation was observed for the relationship between class images and political criticism depicted in Figure 3. There the indirect effects of social inequality follow a more tortuous route toward criticism, although they are much less than the effects of stratification on cynicism. The contrast between the two models is instructive in that it gives some latitude to the assumption that criticism and cynicism do not result from lack of empathy, inadequate information, distrust, or pure misunderstanding. These kinds of influences are present and detectable but do not preempt the explanatory contribution of other factors. For example, level of information correlates very weakly with cynicism (Pearson's $r = .02$) and not at all with criticism ($r = 0$).

Precisely what is the contribution of those other factors? The sign of the path from ideological tendency to cynicism suggests that the latter is more characteristic among the Left. Once the effects of stratification and interpersonal distrust are accounted for, the magnitude of the partial beta for ideological tendency goes up, declining slightly with the addition of political self-image to the model. Yet political self-image and stratification are not related (Pearson's $r = -.04$), while the indirect path from the latter to cynicism changes sign. Apparently none of the strata is unique in its mixture of party sympathizers, independents and apolitical persons; the upper strata are less distrustful and less rightist, and also less cynical. But if neither ideological tendency nor level of interpersonal distrust can alter the magnitude of the direct path from stratification to cynicism, what is one to make of lower-class cynicism?

The answer seems to lie with the parties which may play for lower-class Venezuelans the same role and function that "civic" voluntarism plays for the middle class in the United States (Baloyra and Martz, 1979, pp. 64–74). It



Source: Computed from data collected by Baloyra and Martz (1979, pp. 195–206).

Note: All paths leading to political cynicism are third-order partial betas.

Magnitudes of the original zero-order (Pearson's) correlation coefficients between political cynicism and:

Stratification = −.22
Interpersonal Distrust = .22
Ideological Tendency = −.15
Political Self-Image = .15

Not Represented:

Stratification and Political Self-Image = −.04*
Ideological Tendency and Interpersonal Distrust = −.01*
Ideological Tendency and Political Self-Image = −.09

*Not significant
N = 661

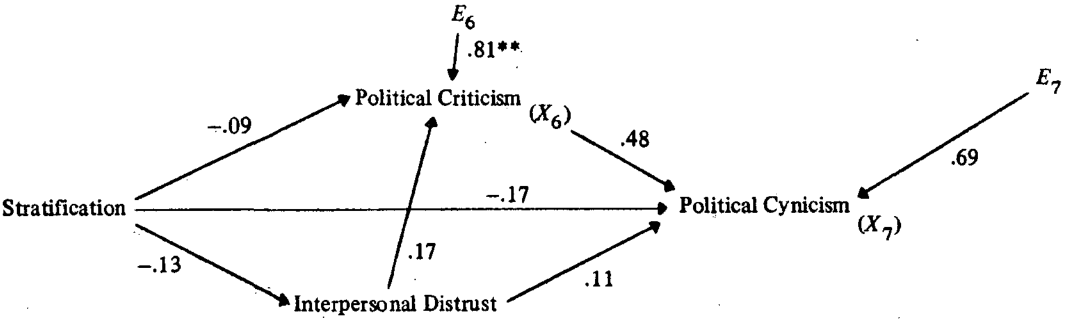
Figure 4. Sources of Political Cynicism in the Venezuelan Electorate

appears that participatory experiences through parties tend to deflate political criticism among lower-class Venezuelans. However, *lower-class cynicism seems beyond redemption by ideological or partisan manipulation in Venezuela.*

There is one additional element, missing in the model depicted in Figure 4, which should be made explicit in order to emphasize the connection between criticism and cynicism. Criticism, measured in aggregate terms, looms very large among the sources of political cynicism presented in the path model of Figure 5. Inclusion of criticism eliminates ideological tendency and political self-image from the explanation, while it cannot displace stratification and interpersonal distrust which carry over from the previous model. Indeed the impact of the former is reduced somewhat, while the impact of distrust is cut more drastically, but both factors must be included in this new version.

The social bases of political criticism and political cynicism may now be evaluated by contrasting the models of Figures 3, 4, and 5. First of all, *the impact of social inequality on political criticism is relatively mild,* although independent of any other causal factor. However, the impact of social inequality on political cynicism is much more pronounced. In addition, the introduction of criticism into the explanation of cynicism, both being related very closely, fails to overshadow the impact of social inequality which must still be included in the model of Figure 5.

The implications of this seem relatively straightforward. Criticism is so generalized, so intertwined with party preference and ideological tendency, and even with the individual's degree of politicization (measured by self-image), that the relevance of social inequality is diminished. In other words, *Venezuelans need not be deprived to be critical but,* regardless of their levels of criticism, *chances are that they feel socially deprived if they are cynical.* The models presented in Figures 4 and 5 are conclusive in this regard. Whether considered in



Source: Computed from data collected by Baloyra and Martz (1979, pp. 195–206).

Note: All paths leading to political cynicism are second-order partial betas.

Magnitudes of the original zero-order (Pearson's) correlations between political cynicism and:

Stratification = –.23
Interpersonal Distrust = .22
Political Criticism = .51

Magnitudes of third-order partial betas between political cynicism and other theoretically relevant variables:

Nominal Party Preference = –.02*
Political Self-Image = .01*
Ideological Tendency = –.04*

*Not significant
**From the solution presented in Figure 3
N = 666

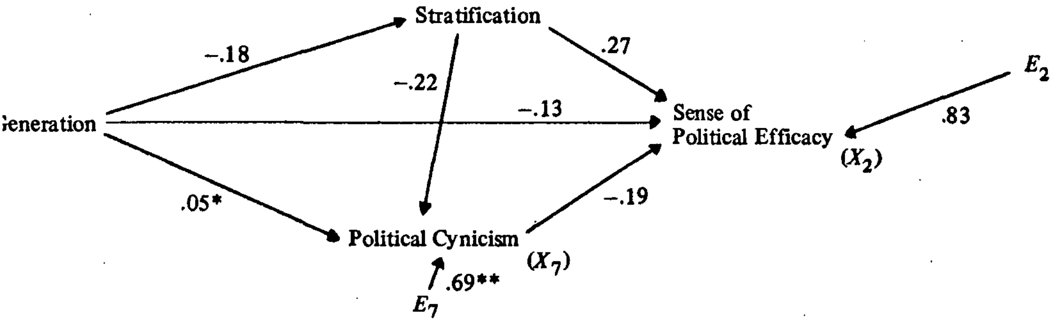
Figure 5. Criticism, Distrust, and Cynicism

erms of criticism, or whether criticism is xcluded from the explanation, the cynic is nown as a socially deprived person. Other actors drop in and out of the explanation, but he element of social inequality remains. In ddition, even though practically everyone in enezuela votes and even though the lower rata tend to be members of political parties nore often than their upper-status counterarts, the upper strata feel more politically fficacious than the lower (Baloyra and Martz, 979, pp. 66–68). These participatory activiies may not bring much amelioration in a ocial sense, leading to more distrust, more political self-images, and more cynicism. In ither case, it is obvious that *political cynicism annot be separated from low social condition n Venezuela.*

Criticism, Cynicism, and Efficacy

It is necessary to emphasize that the distinc- on between criticism and cynicism has been made using items purported to measure "trust in government." This is important because even though our findings are in line with the conventional wisdom concerning the well-documented link between cynicism and low social condition, *cynicism has been measured as very extreme criticism* and not as the less corrosive forms identified as "cynicism" in the literature and treated as "criticism" in this analysis. This reminder intends to underline the fact that criticism appears to be a relatively "normal" part of Venezuelan politics, very much a result of party and ideological oppositions. In addition, there is reason to believe that criticism does not result in any serious decline in extant levels of the sense of political efficacy among Venezuelans.

The path diagram presented in Figure 6 shows that the sense of political efficacy can only be deflated by cynicism, not criticism, and this in conjunction with social condition and position in the life cycle. The model assumes that an individual's sense of political efficacy is



ource: Computed from data collected by Baloyra and Martz (1979, pp. 195–206).

ote: All paths leading to sense of political efficacy are second-order partial betas.

agnitudes of the original zero-order (Pearson's) correlation coefficients between sense of political efficacy and:

Stratification = .34
Political Cynicism = –.26
Generation = –.19

agnitudes of third-order partial betas between sense of political efficacy and other theoretically relevant ariables, with their original zero-order correlations in parentheses:

Interpersonal Distrust = –.04*	(–.11)
Ideological Tendency = –.03*	(–.05*)
Political Self-Image = –.05*	(–.09*)
Nominal Party Preference = –.02*	(–.03*)
Political Agility = –.02*	(.00)
Political Criticism = –.07	(–.17)

*Not significant
**From the solution presented in Figure 5
N = 666

Figure 6. Stratification, Political Cynicism, and Sense of Political Efficacy

determined by age, position in the social structure, and degree of cynicism. More specifically, the sense of efficacy is anchored on stratification or, better, on individual experiences related to social condition. Since aging brings a status deflation later in life, generation is shown to have direct and indirect impacts on the sense of efficacy, although there is no indirect impact through cynicism. Older Venezuelans feel less efficacious because of their generational experiences and declining social status, not because of their cynicism. Cynicism, on the other hand, appears to have more direct impact on the sense of efficacy. Finally, a higher social status reduces criticism while it is accompanied by a greater sense of efficacy.

It could be argued that Almond's and Verba's (1965, pp. 191–94) treatment of cynicism as dependent on a lack of sense of efficacy is equally plausible. However, criticism of administration policies was the only mode of political criticism related to efficacy in the first model (Figure 1), efficacy did not emerge as an important predictor of any of the modes of criticism (Figure 2), and the relationship between criticism and the sense of efficacy disappears when one controls for level of cynicism (Figure 6). Therefore, treatment of the sense of efficacy as a dependent variable seems justified by the findings,[8] and by their theoretical implications. Does this approach improve the theoretical usefulness of "sense of political efficacy?" Unfortunately, it does not.

In part, the problem with the concept of political efficacy is that we seem to know what it goes with—affluence, education, generation, civic norms and so forth (Balch, 1974, pp. 1–2)—but have not been able to solve the trust-efficacy riddle satisfactorily. There is almost universal agreement that the sense o political efficacy ought to be high—like th Dow-Jones average or the parity of the dollar—to preserve democratic stability. In Venezuela where levels of criticism are high, only on mode of criticism could be connected t fluctuations in levels of efficacy. That particular mode—criticism of the policies of th incumbent administration—has been measured in very specific terms. Paige was correct i warning that it may be very difficult t construct a measure of efficacy that is no contaminated by trust (1971, p. 814). However, this discussion has pointed out that wha we need is to conceive of the sense of efficac in its different modes. In addition, the sense o efficacy may not have as much impact o actual political behavior as it may appear.

Cynicism, Efficacy, and Participation

Almond and Verba believed that the sense o political efficacy increases individual satisfaction with the role of participant (1965, p. 202). Balch (1974, p. 1, on Campbell et al., 1954) suggests that the concept of efficacy was first developed to help explain variations in electoral participation. Obviously, it would be impossible to test this hypothesis in Venezuela, given present levels of voting turnout. However, it has been shown that if a system of voluntary suffrage were introduced in Venezuela, those citizens who would be more likely to abstain from voting tend to match the profile of the cynic (Baloyra and Martz, 1976).

In trying to sort out the reasons for this type of non-participation in Venezuela one cannot come up with the sense of political efficacy among them, as suggested by the path model presented in Figure 7. Satisfaction with the role of participant in elections (voter)[9] is influenced by other factors. Notice that the data include second-order partial betas and the original zero-order correlations for the variables included in the model, as well as for those deemed relevant that did not turn out to be so. The model suggests that role dissatisfaction

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[8] Zero-order correlations between efficacy (X_2), criticism (X_6), and cynicism (X_7) are $r_{26} = -.17$, $r_{27} = -.26$, and $r_{67} = .51$, respectively. From the model presented in Figure 6 and the discussion in the text it can be assumed that $P_{76} = .48$ and since $P_{26.7} = -.05$ the "weak" causal ordering appears to be

$$\begin{tikzcd} X_6 \arrow[r, ".48"] & X_7 \arrow[r, leftrightarrow, "-.18"] & X_2 \end{tikzcd}$$

although for the reasons advanced in the discussion it can be assumed

$$\begin{tikzcd} X_6 \arrow[r, ".48"] & X_7 \arrow[r, leftrightarrow, "-.18"] & X_2 \end{tikzcd} .$$

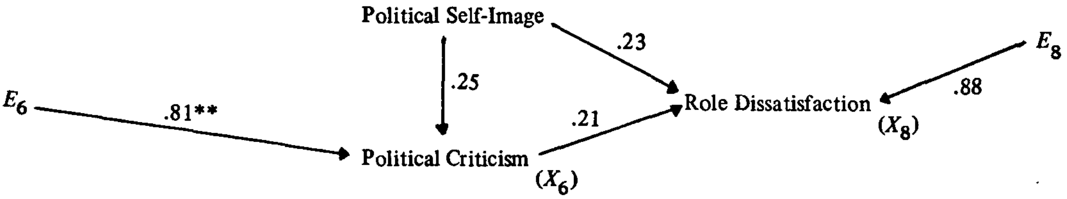
Basically, the implication is that only criticism of government policies and cynicism can deflate the sense of political efficacy, while the context and issue- or actor-connected modes of efficacy that may reduce cynicism are yet to be specified in the literature.

[9] "Satisfaction with the role of participant in elections" was measured in the 1973 survey with the following item: Q. "Look and, if voting was not mandatory, would you vote or not?" Forty-eight percent of the respondents said "yes," they would continue to vote, while an identical percentage said "no," 3 percent answered "depends," and the rest (less than one percent) did not know or refused to answer the question.

originates in criticism (measured in aggregate terms) and in the degree of politicization of the individual (measured in aggregate terms) and in the degree of politicization of the individual (measured by political self-image). Independents and apolitical persons, who also happen to be more critical, as well as critics in general, are the more likely candidates for withdrawal.

The model appears to have a number of important omissions. The more noticeable, as viewed from the perspective of the participation hypothesis and the literature associated with it, involves the absence of political efficacy from the explanation. In other words, people would no longer vote in Venezuela for reasons other than their declining or low sense of political efficacy. There is some association between the sense of efficacy and voting withdrawal (Pearson's correlation of –.12 at bottom) but once self-image and criticism are accounted for, the impact of efficacy on role dissatisfaction is reduced to one half of its original magnitude. Likewise, individual social circumstances, measured by class images and stratification, do not seem to have much relevance. It is not merely deprived people with a low sense of efficacy who would drop out of the system in this fashion, but *primarily people who do not want to play political roles in the first place.*

The fact that criticism and not cynicism is included in the model should not pass without comment. It has been shown that only one mode of criticism has some impact on sense of efficacy but, using the standardized aggregate index of political criticism, one finds this to have as much impact on role dissatisfaction as that of the individual's political self-image. Therefore, criticism has some corrosive effect after all since it contributes to the decrease in participation in elections. However, one should view this fact with some caution since it should be recalled (from Figure 2) that two of the scales of criticism included in the composite index allude to elections and politicians. Since the latter attract the highest levels of criticism found among the Venezuelan electorate, there may be much overlapping between criticism



Source: Computed from data collected by Baloyra and Martz (1979, pp. 195–206).

Note: All paths are first-order partial betas except between the exogenous variables which is a zero-order correlation.

Magnitudes of the original zero-order (Pearson's) correlations between role dissatisfaction and:

Political Self-Image = .28
Political Criticism = .27

Magnitudes of second-order partial betas between role dissatisfaction and other theoretically relevant variables, with their original zero-order correlations in parentheses:

Interpersonal Distrust = –.07	(.13)
Ideological Tendency = –.02*	(–.06*)
Nominal Party Preference = –.05*	(–.15)
Political Agility = –.01*	(.00)
Political Cynicism = .01*	(.22)
Sense of Political Efficacy = –.06	(–.12)
Class Image = –.01*	(–.05*)
Stratification = –.03*	(–.05*)

*Not significant
**From the solution presented in Figure 3
N = 666

Figure 7. Sources of Dissatisfaction with the Role of Participant in Elections

and cynicism with respect to how politicians are evaluated. Consequently, even though 71.3 percent of the electorate are very supportive or highly supportive of elections, a number of them are among the 48 percent who would not vote if they did not have to, perhaps to punish the party politicians.

In summary, it is not who you are (nobody/somebody) nor how efficacious you feel (incapable/capable) but how politicized you are (apolitical/independent/partisan) and how critical you are that will determine whether you continue to vote or not. Obviously, what is required is not a great deal of cynicism, at least by Venezuelan standards, but intense levels of criticism in those modes having to do with parties and politicians. Thus, it is not the sense of efficacy itself but factors which contribute to fluctuations in the sense of efficacy which are more relevant determinants of the decision to participate. In my view, there is nothing "unconventional" about this.

Recapitulation

Survey research has made important contributions to contemporary political science. Evidence accumulated during the last 35 years has allowed us to maintain a record of public reaction to politics in the United States and some of the other contemporary democratic regimes. This tradition has proved its usefulness for comparative politics as well, as evidenced by numerous studies dealing with similar topics in different cultural contexts. Ironically, attempts to interpret ongoing changes in some of the orientations among the publics of many democratic regimes have exposed some of the weaknesses of the assumptions and propositions which have lent theoretical coherence to the field. Nothing remotely resembling a "scientific revolution" is taking place, but the ongoing revaluation offers an ideal opportunity for theoretical discussion and clarification.

A central concern of this analysis has been the use of standard measures of "civic attitudes" in making an analytical distinction between political criticism and political cynicism. The results of the analysis suggest that *political criticism is primarily a consequence of partisan and ideological oppositions,* although not beyond the influence of interpersonal distrust and social inequality. By contrast, *political cynicism appears as a form of criticism that cannot be dissociated from social condition.* The fact that this distinction can be made for Venezuela is especially significant, in light of the contextual and empirical difficulties discussed in this article. Consequently, I believe that the distinction should be easier to make, using more robust measures in less critical contexts.

Some of the theoretical by-products of the analysis ought to lead to reflection about the role of criticism in contemporary democratic societies, as well as to some healthy skepticism about the theoretical relevance of the concept sense of political efficacy. Once it was established that it is more appropriate to treat the sense of political efficacy as dependent on criticism, rather than the other way around, not much relevance can be attached to the concept. Efficacy does not appear to have much impact on participation, while it is shown to fluctuate with individual evaluation of the performance of the government. However, in the United States, with a system of voluntary suffrage, this mechanism (increased criticism-reduced efficacy) may be at work in keeping people from participating in elections.

It goes without saying that all of the above relates to Venezuela and that its application to other cases may result in findings different from those discussed here. However, I would like to stress that the distinction between criticism and cynicism, which may or may not be useful, suggests that there may be a lot to gain from taking a closer look at some of the assumptions purporting to explain relationships which remained relatively invariant during much of the maturation period of the survey approach. It is symptomatic that many of the otherwise excellent attempts to deal with fluctuations in levels of trust and efficacy in the United States center on questions of validity and reliability, and not on the facts of change themselves. In other words, the measures need improvement but there is little question that civic attitudes *have* indeed changed during the last decade. I hope the ability to distinguish cynicism from criticism will prove itself helpful, but remain convinced that political cynicism may always be with us. As for political criticism, it is hard to conceive of a democratic society that would not have a place for it.

Appendix.
Coding Categories of the Independent Variables Used in the Study*

Interpersonal distrust
1. low (1.4)
2. moderate (5.2)
3. high (18.4)
4. very high (75.0)

Nominal party sympathy

1. MEP, *Movimiento Electoral del Pueblo* (3.9)
2. MAS, *Movimiento al Socialismo* (6.4)
3. none (35.3)
4. AD, *Acción Democrática* (22.6)
5. COPEI, *Partido Social Cristiano* (27.2)
6. CCN, *Cruzada Cívica Nacionalista* (3.3)
7. other (1.4)

Ideological tendency

1. Radical Left (15.4)
2. Left (7.5)
3. Center Left (13.0)
4. Center (10.8)
5. Center Right (11.1)
6. Right (42.2)

Social stratification

1. agricultural poor (16.0)
2. manual poor (20.5)
3. manual middle class (24.8)
4. lower white collar (14.8)
5. upper white collar (9.4)
6. professional (14.5)

Political self-image

1. militant, party sympathizer (48.7)
2. independent (19.2)
3. apolitical (32.1)

Size of community of residence

1. rural, less than 5,000 inhabitants (24.5)
2. intermediate, between 5,000 and 24,999 (13.9)
3. urban, between 25,000 and 99,999 (15.6)
4. metropolitan, larger than 100,000 (46.0)

Region of residence

1. Plains (7.4)
2. Andes (11.6)
3. West (14.3)
4. East (16.9)
5. Zulia (11.4)
6. Center (38.4)

Political agility or level of political information

1. attuned (67.0)
2. disoriented (33.0)

Gender role

1. male (49.7)
2. female (50.3)

Religiosity

1. none, agnostic, atheist (3.9)
2. Protestant (2.0)
3. nominal Catholic (58.4)
4. practicing Catholic (35.7)

Generation

1. 18 to 24 (27.5)
2. 25 to 29 (14.6)
3. 30 to 34 (11.1)
4. 35 to 39 (10.3)
5. 40 to 44 (9.0)
6. 45 to 49 (8.0)
7. 50 to 54 (5.2)
8. 55 to 59 (4.9)
9. 60 and over (9.5)

Class images

1. poor (35.0)
2. middle class (65.0)

*Adjusted relative frequencies in parentheses.

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Social Amelioration through Mass Insurgency? A Reexamination of the Piven and Cloward Thesis

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Frances Fox Piven and Richard A. Cloward (1971, 1977) have argued that mass insurgency in the United States, occurring especially between 1964 and 1969, produced a series of responses by government, one of the most significant being massive expansion of welfare rolls. Using data on which they base their claim, this study examines the hypothesis that there is a positive association between social disorders and welfare caseload increases. The conclusion is that associations specified by Piven and Cloward are not supported by the data and a plausible rival hypothesis is offered to explain the massive increases in welfare caseloads.

Social science research that focuses on explanation seldom bridges the gap between this objective and developing strategies for achieving group or social ends. The collaborative work of Frances Fox Piven and Richard A. Cloward is one of the rare examples of research that combines both social theory and social praxis. By analyzing historic movements of social protest, they suggest not only causal explanations of social policy shifts, but also implications for strategies by which the poor can achieve economic and political gains from governments and members of the elite ordinarily inured to their demands. As a significant contribution to the literature on the politics of mass society, their argument deserves more serious attention and more detailed analysis.

Piven and Cloward (1971, 1977) argue that mass insurgency is virtually the only strategy available to the poor for compelling social and economic progress consonant with their interests. Of all the influences that the poor have exerted upon the political structure to improve conditions of the lower classes, only mass disruption and defiance have achieved notable gains. Mob looting, rent riots, and relief insurgency brought expanded relief support and recognition of the cause of unemployed workers in the 1930s. Widespread labor unrest, including strikes, beyond the control of union officials, won concessions to labor power. Mass rioting in the United States between 1964 and 1969 produced a series of responses by government, one of the most significant being massive expansion of the welfare rolls. Piven and Cloward concede that there are inherent risks in pursuing strategies of mass defiance and that historical conditions may not be propitious for their success, but such caveats are diminished by the fact that the limited political influence of the poor depends upon mass protest and disruption of the social and political order. Despite its limitations, enlargement of lower-class political influence is linked to a strategy of escalating "the momentum and impact of disruptive protest at each stage in its emergence and evolution" (1977, p. 37).

This is an innovative thesis in the literature about the politics of mass society. Piven's and Cloward's special contribution is to focus attention less on the causes of mass insurgency than on the potential for political influence by the poor in periods of widespread social and institutional dislocation; less on the causes of disruptions and rebellions than on their outcomes; less on explanations of why men and women rebel than on what happens when they do. Only a passion for organizational and electoral politics, they contend, limits the prospects offered by spontaneous disruption or rebellion. But sometimes the threat of social and political disorders enables the lower classes to wrest economic and political gains from governments and elites. The implications for praxis are clear: "A placid poor get nothing, but a turbulent poor sometimes get something" (1971, p. 338).

In a highly original treatment of the welfare system, *Regulating the Poor: The Functions of Public Welfare,* Piven and Cloward claim empirical support for their thesis. Echoing perceptions of many thoughtful Americans in characterizing the late 1960s as a period of "welfare explosion," they argue that an exponential increase in welfare spending and caseloads (particularly in the Aid to Families with Dependent Children program) was an outcome of historic causal processes, the most proximate and significant being the mass insurgency that characterized the 1960s. According to their analysis, increases in AFDC represented a dramatic shift in structures of welfare institutions that had remained relatively stable for 25 years. Some impetus for this change was provided by community-organizing efforts in major American cities, instigated and largely funded by the Office of Economic Opportunity. However, the *effectiveness* of these efforts was a product of growing social and political disorder that caused governments at federal, state, and local levels to respond with increased welfare caseloads and benefits. "Relief-giving does not increase simply because economic deprivation spreads," they contend, "nor did it increase for this reason in the 1960s" (1971, p. 190). The primary cause was a mobilization of the poor to supplant traditional politics with threats of mass social and political disruptions, to which government responded with more welfare.

Piven and Cloward indicate the appropriate course for testing this "mass insurgency" thesis by linking specific welfare policy changes to the disorders of the 1960s. In accordance with their thesis, the rise in welfare activity should coincide with protests, demonstrations, riots, and other evidence of the breakdown of social legitimacy. Their claim is that mass disturbances and welfare increases can be linked empirically so as to confirm their analysis. This research is an effort to clarify the causal relationships asserted in *Regulating the Poor* and to provide empirical tests of the Piven and Cloward thesis—that during the 1960s mass turbulence led to liberalized social welfare policies as a political response to social and political disorder.

Specification of the Model

Piven's and Cloward's work provides no explicit model clearly linking mass insurgency to social amelioration and change. Consequently, efforts to test their major propositions bear the burden of constructing the chain of causal processes in a manner consistent with their thesis. In their most unambiguous formulation of a causal model, Piven and Cloward state (1971, p. 338): "The terms in which this crisis [the relief explosion of the 1960s] must be explained are economic disruption, large-scale migration, mass volatility, and electoral responses—a sequence of disturbances leading to a precipitous expansion of the welfare rolls." Although the explicitness of this statement is qualified by a lengthy discussion of federal anti-poverty activities as the catalyst for mass volatility and electoral response, it specifies the components of a developmental model in which a clearly defined causal sequence results in specified outcomes—massive increases in AFDC caseloads. With the qualification just noted, this statement appears to provide a basis for operationalizing and testing linkages representing their "mass insurgency" thesis and, thus, an implicit test of their thesis as a whole.

Economic disruptions specified by Piven and Cloward are those occurring during the post-World War II period when a combination of returning veterans, agricultural mechanization, and economic recession displaced thousands of unskilled workers, largely southern and black, into the ranks of the unemployed. The migration of southern blacks into northern industrial cities where they became a new "underclass" was accompanied by a corresponding social dislocation that undermined legitimacy structures and paved the way for spontaneous rebellions that followed (Smelser, 1963; Kornhauser, 1959; Skolnick, 1969). Thus, *effects* of "economic disruption" are expressed as mass migration and the ensuing social and political unrest.

The role of "electoral responses" in the model is more complex. Piven and Cloward argue that a strategy of mass insurgency was successful only because black voters possessed a strategic potential to disrupt the traditional Democratic party coalition of presidential politics. The potential for blacks in northern cities to determine the outcome of presidential elections, either by defections to the Republican party or by withholding their votes from Democratic candidates, destabilized electoral conditions and forced Democratic political leadership of both Congress and the presidency to resolve conflicting interests of northern black and southern white constituencies. The issue was resolved when members of the Democratic party elite chose to respond to the political threat of mass insurgency by forsaking their southern, white interests in favor of solidifying electoral support among black Americans (1977, p. 15ff.).

Specified in this way, "electoral responses" is not a variable, but a constant. The argument that mass insurgency of the 1960s produced gains for the lower classes in the form of expanded welfare coverage implies the tautological conclusion that conditions of the electoral system were such as to insure success of protest strategies. If electoral conditions had not been favorable, the protests would have failed to produce results. Thus, electoral environment constitutes a catalyst specified as constant in its effect throughout the 1960s, which, at least for this period, is not a variable that requires inclusion in the model.

The crucial linkages of the model may be summarized as follows. Social dislocations represented by mass migrations of blacks to northern cities weakened structures that motivated these migrants and their progeny to express their grievances through legitimate social and political institutions. In the 1960s, failures of these institutions to meet the expectations of the poor resulted in mass insurgency that was concentrated primarily in northern urban areas. Confronted with the breakdown of social and political order, governments at all levels—federal, state, and local—attempted to placate, rather than suppress the growing turbulence of the cities. The result was an observed "explosion" of the welfare system in the form of caseloads and benefits.

Effects of Mass Insurgency

In support of their argument, Piven and Cloward present data on changes in welfare (AFDC) caseloads for regions, states, and the 121 major urban counties for the periods 1950–60, 1960–69, and 1964–69, which ostensibly confirm the coincidence of the welfare explosion with major urban disorders. With this data, they demonstrate that while the largest urban counties accounted for less than one-quarter of the national increase in AFDC during the 1950s, these same counties, marked by urban violence, contributed approximately 70 percent of the increase during the 1960s. Most crucial to their thesis is the precipitous increase that occurred after 1964—58 percent compared to 31 percent during the first four years of the decade. "Where" and "when" the largest and most rapid rates of welfare increases occurred constitutes the major supporting evidence that mass insurgency produced political responses leading to increases of the welfare rolls (1971, p. 187).

However, the data cited by Piven and Cloward do not provide strong support for the model. Their contention that the steepest increases in welfare caseloads (217 percent) occurred in the five most populous counties—New York, Philadelphia, Cook County (Chicago), Wayne County (Detroit), and Los Angeles—is based upon a misleading aggregation of the data. While New York and Los Angeles show higher rates of increase than the 116 remaining urban counties, the other three—Philadelphia (123 percent), Cook County (109 percent), and Wayne County (77 percent)—are all substantially below the 116-county average (135 percent). (See Piven and Cloward, 1971, Source Table 2.) In fact, only New York and Los Angeles appear among the top *ten* counties in proportion of increase in AFDC caseloads during the 1960–69 period (see Table 1).

The timing of the largest proportionate increases in welfare (AFDC) programs also leads to somewhat mixed conclusions. Nearly one-third of the urban counties (38) show greater increases during the 1960–64 period than during the period of mass insurgency, 1964–69.[1] Over one-fifth of these same coun-

Table 1. Top Ten Urban Areas in Proportionate Increases of AFDC Caseloads and Rank Order According to Increased Incidence of Robbery, 1964–69

Urban Areas Ranked by Proportionate Increase in AFDC Caseload	Percent AFDC Caseload Change	Rank of Increase in Robbery
1. Lubbock, Texas	300	117
2. Columbus, Ohio	225	70
3. Anaheim, California	195	67
4. Dallas, Texas	194	16
5. Atlanta, Georgia	184	65
6. Austin, Texas	180	60
7. Los Angeles, California	145	22
8.5. Savannah, Georgia	144	38
8.5. Yonkers, New York (Westchester County)	144	1.5*
10. New York City	137	1.5*

Source: Frances Fox Piven and Richard A. Cloward (1971), *Regulating the Poor,* New York: Random House, Source Table 2.

*The Uniform Crime Reports do not distinguish between New York City and Westchester County in reporting crimes by urban areas.

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[1] Feagin and Hahn (1973) note that the focus on riots occurring 1964–69 is largely a function of mass

ties (25) experienced higher growth rates of AFDC caseloads between 1950 and 1960 than during the 1960s (see Piven and Cloward, 1971, Source Table 2). In addition, Piven's and Cloward's attempt to link unemployment to the mass insurgency-welfare explosion process is undermined by data showing that the unemployed parent component of AFDC, which specifically provides relief to the unemployed, increased by substantially *larger* amounts (and rates) from 1960 to 1964, than during the 1964–69 period (see Piven and Cloward, 1971, Source Table 3).

These anomalies in Piven's and Cloward's data challenge their thesis that "the contemporary relief explosion was a response to the civil disorder caused by rapid economic change" (1971, p. 196). The data do not unequivocally confirm that the impetus toward liberalization of welfare was concentrated in the largest urban areas, nor that it had its beginning in the onset of mass disorders after 1964. There is clearly a need for further analysis and testing of these basic claims.

At least two of the claims advanced by Piven and Cloward seem amenable to more formal empirical tests. The first is that the areas of greatest social and political disorders were those that experienced the largest amounts of black immigration as a result of the post-World War II economic dislocations; second, that the most rapid rises in welfare (AFDC) caseloads coincided with those areas marked by civil disorders. These propositions may be tested using the 121 urban counties for which Piven and Cloward provide data. The measure of the dependent variable is the proportionate increase in AFDC caseloads during the period in which mass disorders occurred in urban areas of the United States (1964–69). Percentage changes in AFDC caseloads during this period are consistent with an emphasis on *dynamics* of welfare increases since the absolute magnitude of increases is generally a function of population size.

The intervening variable, *mass insurgency*, is operationalized as incidents of urban violence. Piven and Cloward suggest that "urban violence" refers not solely to mass urban disorders, but also to crimes of violence occurring especially, but not exclusively, in large urban areas. Frequent invocations of "crime in the streets" to provoke political responses, whether of a repressive or ameliorative type, probably blurred distinctions between mass disorders and individual crimes of violence. Thus, an indicator of "urban violence," as Piven and Cloward suggest (1971, p. 226), is a rise in serious crime, especially robbery and burglary. A positive relationship between these crimes and increases in welfare caseloads in urban areas provides support for the Piven and Cloward thesis.

The tests that follow link changes in the incidence of robbery and burglary in urban areas to the percent change in welfare caseloads during the period 1964–69.[2] Table 2 indicates the strength of the relationship between increase in incidence of robbery and burglary and proportion of increase in AFDC caseloads over the 1964–69 period. Neither changes in the absolute incidence of these crimes nor changes in their rate of increase show any relation to welfare caseloads. In every case, the proportion of variance explained is virtually zero. (Although not shown in Table 2, coefficients of regression are not significantly different from zero.)

Anticipating that inclusion of southern urban areas might distort the relationship, I performed the same analysis for the 70 nonsouthern counties included in the sample. One observes only minor differences in the outcome (Table 2). Clearly, no significant relationship exists between increases in crime rates, e.g., robbery and burglary, and AFDC caseload increases during the period in question. In this respect, the null hypothesis cannot be rejected and the data do not lend support to the Piven and Cloward argument.

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media attention. Most of the major riots occurred during this period, however, culminating in the riots of 1968. In any case, it is a useful period for examining the Piven-Cloward thesis and is consistent with their interpretation of events.

[2] Data on incidence of robbery and burglary are taken from the *Uniform Crime Reports* (1964, 1969). Data on these indicators are available for all but one (Montgomery, Alabama) of the 121 urban areas examined by Piven and Cloward. In the *UCR*, several of the urban counties are aggregated with adjoining large cities. Since these data would be identical for incidence of crime (e.g., New York City and Westchester County, or San Francisco and Oakland) but different for welfare caseload increases, inclusion of *both* areas would bias the analysis in favor of the null hypothesis. In these cases, the smaller areas have been dropped from the analysis, a procedure that reduces the sample size by an additional 12 units leaving 108 urban areas for analysis. The *UCR* do not distinguish between "robbery" and "armed robbery." The definition of the former includes "armed robbery where a weapon is used and strong-arm robbery . . . such as mugging, yoking, etc." In every case, it is considered a violent crime.

An examination of the relationship of inreases in crime to welfare caseloads is hardly a efinitive test of the Piven and Cloward thesis. more convincing test of the relationship etween mass disorders and welfare increases equires examination of the link between urban iots occurring in the 1960s and corresponding icreases in AFDC caseloads. One way of esting the thesis that mass insurgencies prouced social amelioration in the form of libralized welfare policies is by attempting to nswer the question: were the urban areas arked by violence the same as those characerized by the largest increases in welfare aseloads? The question can be plausibly anwered by comparing proportionate increases in elfare among urban areas in which serious isorders occurred to urban areas in which they did not. Fortunately, Piven and Cloward provide a list of urban areas in which "serious disorders" occurred, i.e., those marked by violence lasting more than one day, with at least some fires and rioting, and the use of state police to quell the disturbance (1971, pp. 239–40). In addition, they provide a list of comparable urban areas in which little or no rioting took place.

A comparison of means of the rate of welfare increases between 1964 and 1969 for the two types of areas is presented in Table 3. While urban areas in which riots occurred increased welfare caseloads at a larger rate than those not experiencing riots (78 to 67 percent), the difference is not significant. Again, the null hypothesis cannot be rejected and the evidence fails to support the hypothesis that mass

Table 2. Proportion of Variance in AFDC Caseload Increase Rates Explained by Increases in Incidence of Robbery and Burglary in 108 Urban Counties and 70 Non-Southern Counties, 1964–69*

	(r^2)
08 Urban Counties	
Change in incidence of robbery with percent increase in AFDC caseloads	.0103
Change in incidence of burglary with percent increase in AFDC caseloads	.0159
Rate of increase in robberies with percent increase in AFDC caseloads	.0001
Rate of increase in burglaries with percent increase in AFDC caseloads	.0004
0 Non-Southern Urban Counties	
Change in incidence of robbery with percent increase in AFDC caseloads	.0188
Change in incidence of burglary with percent increase in AFDC caseloads	.0124
Rate of increase in robberies with percent increase in AFDC caseloads	.0045
Rate of increase in burglaries with percent increase in AFDC caseloads	.0062

ource: Frances Fox Piven and Richard A. Cloward (1971), *Regulating the Poor,* New York: Random House, ource Table 2; Federal Bureau of Investigation, *Uniform Crime Reports,* 1964 and 1969.

*No regression coefficients are significantly different from zero at the .05 level. Incidence refers to the numer of crimes per 100,000 population. Rate of increase is the proportionate change in AFDC caseloads or in icidence of crime, respectively.

Table 3. Test for Differences of Mean Percent Increases in AFDC Caseloads between Urban Areas in Which Riots Occurred and Those Where Riots Did Not Occur between 1964–69

	Number of Cases	Mean Increase	Standard* Error
rban areas in which riots occurred	23	78.30%	9.379
rban areas in which riots did not occur	26	67.38%	9.990

t-value = 0.80
df = 47
One-tail Probability = .215

urce: Frances Fox Piven and Richard A. Cloward (1971), *Regulating the Poor,* New York: Random House, urce Table 2 and pp. 239–40.

*F-value for difference of varaince, p > .50.

insurgency in the form of urban riots produced ameliorative political responses in the form of increased welfare caseloads.

Black Fomentation of Mass Insurgency

The specified model suggested by Piven's and Cloward's analysis implies that the pattern of increases in black population as a result of mass migrations is associated with rises in all forms of social disorders, including rises in crime and mass violence. It is difficult to obtain data on migration to specific urban areas for the 1950–60 period and Piven and Cloward do not provide data necessary for testing the relationship between black migration and urban disorders. If one is willing to assume that changes in nonwhite population are representative of migration patterns, even though some o this increase is due to births, the relationshi can be tested with readily available census data The relationship between increases in nonwhit population and social disorders represents linkage inferred by Piven and Cloward—tha blacks were responsible for fomenting specifi disorders that led to increases in welfare (1971 pp. 222, 226).

A test of the hypothesis that changes i nonwhite population between 1950 and 196 contributed to changes in incidence of crime i.e., robbery and burglary, is presented in Tabl 4. Based upon the same urban areas used fo Table 2, coefficients of determination (r^2 indicate that there is virtually no association o

Table 4. Proportion of Variance in Incidences of Robbery and Burglary Explained by Increases in Percent Nonwhite Population for 108 Urban Counties and 70 Non-Southern Counties, 1964–69*

	(r^2)
108 Urban Counties	
Percent increase in nonwhite population with change in incidence of robbery	.0249
Percent increase in nonwhite population with change in incidence of burglary	.0161
Percent increase in nonwhite population with rate of increase in robberies	.0144
Percent increase in nonwhite population with rate of increase in burglaries	.0110
70 Non-Southern Urban Counties	
Percent increase in nonwhite population with change in incidence of robbery	.0344
Percent increase in nonwhite population with change in incidence of burglary	.0083
Percent increase in nonwhite population with rate of increase in robberies	.0258
Percent increase in nonwhite population with rate of increase in burglaries	.0102

Source: U.S. Bureau of the Census, *U.S. Census of the Population,* 1950, 1960; Federal Bureau of Investigatio *Uniform Crime Reports,* 1964, 1969.

*No regression coefficients are significantly different from zero at the .05 level. All correlation coefficien are negative. Incidence refers to the number of crimes per 100,000 population. Rate of increase is the propo tionate change in incidence of crime.

Table 5. Test for Differences of Mean Percent Increases in Nonwhite Population, 1950–60, between Urban Areas in Which Riots Occurred and Those Where Riots Did Not Occur between 1964–69

	Number of Cases	Mean Increase	Standard[1] Error
Urban areas in which riots occurred	23	83.1344%	13.092
Urban areas in which riots did not occur	26	65.6982%	7.083

t-value[2] = 1.21
$df = 47$
One-tail Probability = .117

Source: Frances Fox Piven and Richard A. Cloward (1971), *Regulating the Poor,* New York: Random House pp. 239–40; U.S. Bureau of the Census, *U.S. Census of Population:* 1950, 1960.

[1] F-value for difference of variances, $p < .01$.
[2] T-value based on pooled variance estimate.

nonwhite population increases with rises in crime during the 1964–69 era. Again, removing from the sample southern urban areas in which nonwhite populations were relatively stable or declining does not markedly change the result.

When one examines the differences between urban areas marked by riots or other mass disorders and those experiencing little or no violence, the results still offer little support for the Piven-Cloward thesis (Table 5). Nonwhite populations in urban areas experiencing riots during the 1964–69 period grew at a faster rate (83 percent) than in those experiencing few or no disorders (66 percent). Again, however, an analysis of the difference of means indicates that this difference is well within chance probabilities. The standard error of the two groups this time indicates that they may be drawn from different populations. Substantively, this means that another variable may be a factor, but neither the analysis nor the data permit more than speculation as to the cause of this difference. The null hypothesis that there is no relationship between changes in nonwhite populations of urban areas and urban disorders of the 1960s cannot be rejected.[3] In summary, neither of the relationships inferred by Piven and Cloward—that increases in nonwhite populations in northern cities led to urban disorders or that the rise of urban disorders led to increases in welfare caseloads—is supported by the analysis.

Plausible Alternative Explanations

Piven and Cloward do not argue that mass insurgency in the 1960s produced substantially higher benefits levels for persons already receiving welfare. They argue the opposite—that a declining rate of benefits increases in the 1960s was accompanied by an exponential increase in the welfare rolls. While average welfare payments during the 1950s increased by half, caseloads increased by only 17 percent, but in the 1960s a one-third increase in benefits was associated with more than a doubling of the welfare rolls (Piven and Cloward, 1971, p. 191).

In fact, the position of the poor may have been much worse than Piven and Cloward suggest. The total and average income gap of poor persons, even among those receiving government assistance, widened steadily between 1965 and 1972, relative to the rest of American society (Plotnick and Skidmore, 1975, p. 86). When increases in welfare grants, 1960–70, are adjusted for inflation, 22 states show declines in average monthly grants to AFDC recipients during this period. When "equity" considerations are taken into account, i.e., the proportion of average per capita income provided by AFDC grants, 44 states show declines over this period (Albritton, 1976, p. 374).

Finding no relationship between patterns of social disorder and welfare increases, can one propose rival hypotheses consistent with the dramatic rise in welfare caseloads and a corresponding decline in real and relative welfare benefits that occurred during the 1960s? One such hypothesis is that the rise in caseloads (AFDC) was a *response to innovations* and adjustments in federal welfare assistance structures by amendments to the Social Security Act adopted in 1965. These amendments (unnoted by Piven and Cloward) not only established a program of comprehensive medical coverage for the poor (Medicaid), but they also provided the first increase in the federal matching share of AFDC grants since 1958. One major provision made indigent patients in mental hospitals and tuberculosis institutions eligible for welfare assistance. Thus, it became in the interest of states to ensure that these persons were added to the rolls to pay for their maintenance and care. Effective July 1, 1965, states were permitted to provide *retroactive* assistance to AFDC families with children over 18 years of age who were still attending school or with minor children earning income. Another major incentive to welfare eligibility at the time was a provision that, for medical coverage (Medicaid), the federal government's reimbursement to states should be not less than 105 percent of the matching federal share of medical assistance at the end of 1965. Under these incentives, states had an interest in getting as many people onto welfare rolls as possible to maximize federal support of state programs in future years.

The increase in federal share of the reimbursement to states is, ironically, not an incentive to increasing welfare grants, but to expanding further the welfare rolls. There are very real fiscal incentives attached to increasing caseloads rather than grants levels and most states show that they understand this arrangement. The reimbursement formula provides a relatively high rate of federal matching funds for welfare grants (as high as 5/6 of the cost) up to a maximum. For states with low levels of welfare grants, the federal government may be considered a fiscal resource by which a state can return five dollars to the state for every one invested in the welfare system. As grants levels exceed the maximum, this incentive declines sharply, but most states, even those with higher benefits, seem to understand this intriguing structure of the welfare system and allocate available funds for increasing caseloads rather than grants.

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[3] This finding agrees with results of a study by Seymour Spilerman (1970) in which the zero-order correlation of percent change in nonwhite population with number of disorders was $r = .04$, for a much larger sample that included even minor racial disorders. Spilerman found that absolute number of blacks was the only significant predictor of race riots—certainly the necessary, if not the sufficient, condition for racial disturbances. The data are, thus, more consistent with alternatives to the migration-social breakdown thesis. See Feagin and Hahn (1973).

The pattern of increases in the welfare rolls during the later 1960s is consistent with a model that emphasizes effects of this legislative initiative at the federal level. It is neither coincidental nor inconsequential for the model that this legislation was a product of the 89th Congress. The Johnson landslide of 1964 produced a Congress determined to advance the interests of the poor. Amendments to the Social Security Act of 1965, signed into law days before the Watts riots, provided important changes in benefits, a mammoth rise in welfare spending, and a larger aggregate of resources to meet the needs of the poor. The result was a rapid expansion of the scope of welfare assistance in the form of increases in AFDC rolls, while cash welfare benefits declined.

Some tentative empirical evidence supports this interpretation of the data. When general growth trends are controlled, abrupt increases in AFDC spending by state and local governments coincide with implementation of the 1965 legislation, after which the course of spending for this category of welfare assistance recedes (Albritton, 1976, pp. 384–85). Similarly, when population size and federal welfare spending are controlled in equations estimating AFDC caseloads, 1960–70, using states as the unit of analysis, one observes a discontinuity in parameters as an abrupt increase that coincides with implementation of these amendments to the Social Security Act. Thus, the pattern of welfare activity during this period shows increases consistent with a trend established before 1965, interrupted by a change in level coinciding with the federal legislation (Albritton, 1975, pp. 61–64).

This perspective on the data, at variance with the Piven and Cloward thesis, suggests an explanation of why no significant relationships appear between AFDC caseload increases and the mass insurgency of the 1960s. Caseloads were largely a function of changes in the structure of welfare policy relationships between the federal government and the states, while the riots and other social disorders had different origins. Absence of relationships between urban violence and the "welfare explosion" suggests independent causation of these two effects.[4] In any case, the riots of the 1960s, whether or not they represented calculated political strategies, appear to have had few, if any, direct consequences for expansion of the welfare system.

Conclusions

The foregoing analysis represents an effort to examine the Piven and Cloward thesis that mass insurgency in the 1960s produced gains for the poor in the form of large-scale additions to AFDC caseloads. Analysis of the data which they claim provides empirical support for this thesis indicates no significant relationships between social disorders (such as riots or crimes) and increases in welfare caseloads, or between increases in nonwhite populations and social disorders in urban areas. In both cases, no association was found that could not be explained by chance. The conclusion is that the relationships specified by Piven and Cloward are not supported by the data in this form of analysis.

A failure to falsify the null hypothesis in this analysis, of course, does not prove that such relationships do not exist. Nevertheless, to the degree that the model as operationalized here accurately represents linkages argued by Piven and Cloward, results of the analysis cast considerable doubt on their conclusions. At the very least, they require a fundamental reconsideration of the way in which protest strategies can be said to accomplish social amelioration and change.

Events of the 1960s and the data advanced by Piven and Cloward are at least equally consistent with an alternative thesis—that the causes of the "welfare explosion" lie in welfare policy innovation and change preceding the period of pronounced mass insurgency that characterized the later decade. Amendments to the Social Security Act extended welfare benefits to millions of new recipients *and* offered incentives to states to admit them to the rolls. The data clearly are more consonant with an explanation holding that the riots and the welfare explosion" are independent of one nother and independently caused.

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[4] The reader should consult Miller, Bolce, and Halligan (1977) for a sophisticated version of the "relative deprivation" thesis with findings that would lend credence to this speculation. This and other explanations are cogently examined in Feagin and Hahn (1973). The general conclusions of the latter are similar to those of this study—that riots produced little, if any, social amelioration.

Did mass disruption and protest succeed as a olitical strategy in winning concessions for the oor? Perhaps the spectre of riots in some cities aused state and local officials to make some eforms before riots occurred in their areas. ery likely the protest movements of the 960s, of all types, gave considerable impetus o efforts at ending racial discrimination against lacks, particularly the black middle class. But or the poor? No. The specific structures that ontributed to the "welfare explosion" probaby antedated the most significant period of rban disorders. For the poor, as well as for hose on welfare, their "prize" was a gradual vorsening of position relative to the rest of merican society as they lag even further ehind nonpoor Americans in a generally exanding economy.

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Electoral Instability, Civil Disorder, and Relief Rises: A Reply to Albritton

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There are two serious flaws in Robert Albritton's challenge to the argument of our *Regulating the Poor* (1971). He tested the wrong theoretical relationships. Then, for the relationships he did test, he used the wrong data.

Testing the Wrong Relationships

In *Regulating the Poor,* we argued that the welfare explosion of the 1960s had its roots in the problems created for the national political system, particularly for the national Democratic party, by economic and demographic changes. Agricultural modernization in the South, followed by mass migration, eventually generated a rising tide of mass volatility, both at the polls and in the streets, which in turn destabilized the regional coalition of the national Democratic party. In the 1960s, the party's response was to cope with these instabilities with a series of measures, ranging from civil rights legislation to urban service programs; one consequence of the urban service programs was to help set off the welfare explosion at the local level. In our analytical scheme, the relationship between mass volatility and the welfare explosion was *indirect;* it was mediated by complex national electoral conditions.

Given the main elements of this analysis, the proper relationships for testing are apparent. One set of relationships is between unstable voting patterns and mass protest, on the one hand, and the national Democratic party's "New Frontier" and "Great Society" legislative and administrative initiatives, on the other. Another set of relationships is between these policy responses and the rising welfare rolls. In short, did mass volatility help explain a series of national policy initiatives that in turn contributed significantly to the subsequent rise in the welfare rolls?

Albritton short-circuited these indirect relationships, and proceeded as if we had said black crime and rioting alone led directly to welfare rises, locality by locality. If we had thought the matter so simple, we would not have troubled to make a detailed analysis of the complex interaction among economic and demographic change, mass volatility, and national legislative responses, and then between national legislative responses and the subsequent local welfare rises.

Only by tossing aside our entire analysis of national electoral politics could Albritton treat "mass volatility" as equivalent to crime and rioting among blacks. In fact, we included voting instabilities in that term, and we gave voting instabilities great weight. Agricultural modernization drove southern rural blacks northward to the urban strongholds of the national Democratic party, with the consequence that a civil rights bloc formed in the northern wing of the party. The South responded with outrage and voting defections. The resulting regional fissure was first visible in the Dixiecrat revolt of 1948, and it worsened in the elections of 1952, 1956 and 1960 (it also worsened because industrial modernization in the South was thrusting up a new middle class whose political sympathies tilted toward the Republican party). The volatility in black voting patterns which then appeared in the election of 1956, and to a lesser extent in 1960, compounded these problems. Perhaps the national Democratic party could survive defections among southern whites, or among blacks, but it could not survive defections among both.

Civil disorder, welling up in the context of electoral instability, aggravated the national Democratic party's problems all the more. Nor was civil disorder limited to crime and rioting, or just to one contending racial group. Blacks mounted organized protests, exemplified in the southern civil rights movement and the northern welfare rights movement. Southern whites also rose up in protest, ranging from the emergence in the mid-1950s of a movement of "massive resistance" led by the political leaders of the South, to the southern white mob and police violence that erupted at the same time and persisted for ten years.

These various expressions of mass volatility were treated as interactive in our analysis; discontent first took form in the voting booths, and then in the streets, but each manifestation tended to reinforce the other. When leaders in the northern wing of the party gave legitimacy to civil rights aspirations in order to ensure the allegiance of an enlarging black constituency, they helped spur the emergence of black protest. And when leaders in the southern wing denounced federal action on civil rights in order to mobilize southern voters against the national Democratic party (the strategems were those of unpledged electors, presidential Republicanism, third-party threats, etc.), they gave legitimacy to white protest. From 1948 onward, each successive episode of voting instability exacerbated civil disorder, and each successive episode of civil disorder exacerbated voting instabilities. To say that the national Democratic party, beginning in the late 1940s, was literally being pulled apart along regional lines by these oppositional movements is not an overstatement.

With the approach of the presidential election of 1960, regional divisions had to be repaired, even while civil disorder was accelerating. John Kennedy's campaign, which emphasized civil rights and poverty, signaled the direction of the party's strategy. As black and white insurgency spread, the Democratic administrations of the 1960s promulgated the measures to restore both electoral stability and civil order, and by the end of the decade, these measures had more or less succeeded. Civil rights legislation imposed political modernization upon the South, with the consequence that white mob violence subsided and the civil rights movement was channeled from mass protest to electoral politics; and the enfranchisement of millions of blacks whose allegiance could be counted upon enabled the Democratic party to shore up its southern regional base. In the urban ghettos, quiescence was gradually restored by new federal urban programs which simultaneously provided services to the black poor and hired the black militants. These new urban programs also helped fuel the welfare explosion, for they both enlarged the volume of applicants for welfare, and pushed up acceptance levels by weakening the capacity of the welfare system to fend off new applicants. In turn, more liberal dispensation of relief contributed to the ebbing of civil disorder in the ghettos.

Of all the new service programs, we considered the federal anti-poverty effort the most significant, and we devoted an entire chapter to documenting the various ways in which anti-poverty activities spurred welfare expansion. For one thing, the new "community action" agencies provided expert information and assistance to those seeking welfare benefits, thus helping to enlarge the volume of applications (up from 500,000 in 1960 to more than 1,000,000 annually, beginning in 1968). Litigation was undertaken by the legal services program which successfully overturned many of the laws and rules disqualifying poor families (residence laws, employable-mother rules, man-in-the-house rules, etc.), so that whole categories of poor people became eligible for assistance (especially in the South, where these exclusionary rules were more vigorously enforced). In addition, anti-poverty agencies provided resources facilitating the emergence of "welfare rights" organizations whose "direct action" tactics generated pressure on welfare officials to make more permissive eligibility decisions. Under the impact of litigation and protest, acceptance rates shot up (from 55 percent in 1960 to 70 percent in 1968). The result was the welfare explosion. Nor were we alone in reaching the conclusion that it was the new service programs, and the anti-poverty program in particular, that did so much to generate the welfare explosion. Among others, the Maryland State Department of Public Welfare, aghast at the fact that its rolls first doubled and then trebled, said in a report issued in the late 1960s that

> when people are everywhere encouraged to make use of public welfare—by publicity, by action workers on their own block, by teachers of their children in school, by their doctors and by any social agency they happen to have contact with, a surge of response must be expected. It is the contention of this report that the increase of AFDC caseload reflects this response to the anti-poverty effort . . . and that the poor families of this state . . . encouraged by the national effort to do something about poverty have responded to the use of public services . . . in numbers heretofore unequalled (cited in *Regulating the Poor* [1971], p. 331).

The sole justification given by Albritton for neglecting our emphasis on national electoral analysis is that "the electoral environment [was] constant in its effects throughout the 1960s," while the welfare rolls only went up steeply in the second half of the decade; in other words, a constant cannot explain a variation. To be sure, both the Kennedy and Johnson administrations were "favorable" to the poor, but that hardly means that the national electoral environment was constant. In the first two years of Kennedy's term, the intensifying threat of southern electoral defections, together with the great power of the southern congressional delegation, prevented Kennedy from making good on either his civil rights or anti-poverty campaign rhetoric. It was only as civil rights demonstrations reached alarming proportions in 1963 (provoked by the events in Birmingham) that Kennedy, and then Johnson, set in motion the initiatives leading to the civil rights acts of 1964 and 1965, and to a series of service programs for the ghettos. The legislation establishing the community mental health centers, the anti-poverty program, aid to school districts with poor children, and the medicaid program were all passed between 1963 and 1965, by which time civil disorder had broken out in the North as well.

The implementation of these programs was not "constant," either; it was shaped and reshaped by the continuing political problems of the national administration. Even as these programs were enacted, rioting and demonstrations worsened both in the North and South, and among both blacks and whites. By mid-decade, the issues of caste, discrimination, and poverty had come to the very fore of national and local politics. In the vortex of these forces, anti-poverty staff became progressively more militant on behalf of poor minority constituents in their dealings with local agencies of government, whether the police, urban renewal authorities, or welfare officials. Thus anti-poverty attorneys had at first shied away from lawsuits that would antagonize local officials; by 1966, the policies and practices of housing authorities, health agencies, and welfare departments were being forcefully and often successfully challenged in the courts. At the same time, anti-poverty staff began to organize the minority poor for direct action against local agencies. Moreover, the federal guidelines being written in this period, and the federal review teams which oversaw their implementation, gave tacit encouragement to these activities, even to the extent of tolerating the mobilization of the poor for mass protests and demonstrations. Mayors, county officials, and governors complained bitterly and vocally that the federal government was financing an assault on them, and they were not wrong. Nor, in the end, could they withstand the assault, and so they too made concessions, whether in the form of the greatly expanded hiring of blacks or the more liberal giving of relief.

There is another, and simpler, sense in which the electoral environment cannot be treated as constant throughout the decade. The policies promulgated in response to the rising tide of mass volatility did not have an immediate impact on any local agency, including the welfare agencies. This was not the Great Depression: here we had no Roosevelt calling upon Congress immediately after his inauguration to enact emergency relief legislation which, within 18 months, would bring some 20 million Americans onto the relief rolls. Instead we had successive administrations which secured the enactment of one program after another, some of which slowly came to have an impact on the relief rolls. Thus it took time for a legislative strategy to evolve, it took time for that legislation to be implemented at the local level, and it took still more time for the reverberations of the new service programs to be felt in rising welfare application rates, acceptance rates, and then in caseload statistics. The anti-poverty program was framed by Kennedy in 1963, enacted under Johnson in 1964, implemented in 1965 and 1966, and only then did its effects begin to be felt. It was 1967 before class actions initiated by anti-poverty attorneys challenging exclusionary welfare regulations reached the lower federal courts, and it was later still before the Supreme Court ruled on them and the Department of Health, Education and Welfare rewrote its guidelines in conformity with the new court rulings. Even then, many a state and local welfare department remained out of conformity until anti-poverty lawyers and welfare rights organizations brought pressure on HEW to bring recalcitrant departments before "compliance hearings." The impact of the "New Frontier" and "Great Society" programs was thus cumulative, not precipitous.

If these were the main arguments of *Regulating the Poor,* how could anyone who set out to develop empirical tests simply correlate crime and riots with welfare caseload changes? We surmise that Albritton was led astray by our emphasis on the historical role of political disorder in producing relief liberalization. We were prompted to write *Regulating the Poor* because we thought that historians of relief had failed to see that it was not just widespread unemployment, but political unrest among the unemployed, that forced the initiation of relief programs or their periodic expansion, and we reviewed evidence going back four centuries in Europe to develop this thesis. We thought the United States in the 1950s provided still more evidence. Millions were being forced off the land and many remained unemployed in the cities during the "rolling recessions" of the period, but the relief rolls rose by a mere 17 percent, a net increase of only 110,000 families, during the entire decade. It was only with the eruption of widespread civil disorder in the 1960s that the relief rolls exploded, leading us to argue (1971, p. 196) that "the contemporary relief explosion was a response to civil disorder caused by rapid economic change." It was sentences such as this one that Albritton seized upon in justifying his simplistic correlational approach. But in doing so, he ignored a major qualification. We did *not* argue that insurgency among the unemployed always, or even usually, produces relief liberalization, as the closing sentence of *Regulating the Poor* emphasizes: "The moral seems clear: a placid poor get nothing, but a turbulent poor *sometimes get something*" (1971, p. 338).

In other words, if mass insurgency is a precondition of relief expansion, it is not a sufficient one. There is the further question of the conditions determining how the ruling stratum will respond. If a regime commands the allegiance of other groups not disaffected by economic changes, then it may be free to ignore or repress the disruptors. But when civil disorder reverberates upon a regime whose grip has weakened, perhaps as a consequence of the same economic instabilities that provoke the poor to insurgency, it is more likely that concessions will be made, such as giving relief. Our analysis of the Democratic administrations of the postwar period was intended to demonstrate that a welfare explosion occurred in the 1960s precisely because widespread civil disorder coincided with the growing vulnerability of those regimes; to be sure, rioting was one source of that vulnerability, but it was not decisive.

Correlating the Wrong Data

If the main arguments of *Regulating the Poor* dealt with national politics, there is still local politics. And on the local level, our general thesis would lead us to expect to find some correlation between rioting and increases in the Aid to Families with Dependent Children (AFDC) rolls. The difficulty is that the relationship between disorder and welfare expansion in particular localities was mediated by the complexities of the national political dynamics we have described, and which Albritton's correlations ignore.

At a minimum, in accounting for variations in correlations locality by locality, one would want to know something about the role that the locality played in a national administration's political fortunes, which has to do in turn with both local voting patterns and the relationship between the national party and the local party. Thus the fact that Chicago's Mayor Daley continued to produce stable majorities, including stable black majorities, led the national administration to tailor its ghetto programs in Chicago to the requirements of the Daley organization. Daley had, after all, been in sufficient command of the local political exigencies created by migration and unemployment to permit the AFDC rolls in Chicago to rise by an exceptional 80 percent in the 1950s. Correlatively, he remained in sufficient command in the 1960s to moderate the impact of the federal programs and to limit the expansion of the welfare rolls to a mere 107 percent at a time when the rolls in New York City and Los Angeles were quadrupling. One would also have to take into account variations in the structure of state and local AFDC programs, for the pattern of welfare increases depended in part upon the specific mechanisms relied upon to exclude the poor from aid. Thus southern AFDC systems were notorious for the web of laws and rules which simply barred whole categories of the poor outright, while in the North a formally more liberal system relied more upon bureaucratic mechanisms, such as discouraging waiting periods or humiliating investigations. It follows that the increases in the rolls in the southern states were much more dramatically affected by anti-poverty litigation, which knocked down exclusionary statutes all at once in the late 1960s, while the rolls in the northern states were pushed upward somewhat more gradually as a result of the myriad pressures generated by the federal programs on the welfare bureaucracies.

If developments in the national polity could be disentangled from developments in the urban polity, an analysis of the relationship between rioting and welfare increases from city to city would indeed be interesting. Great care has to be taken in marshalling the appropriate data, a point which brings us to the second large flaw in Albritton's critique. He employed the wrong data.

To test the relationship between civil disorder and the welfare explosion obviously requires that riots be correlated with *post*-riot welfare increases; instead, Albritton mainly used a *pre*-riot measure of changes in the AFDC rolls. Roughly three-quarters of the riots throughout the decade occurred just in 1967 and in 1968. In these two years alone, more than 300 persons were killed, some 10,000 injured, and 100,000 arrested. But Albritton's measure of caseload changes, defined as the average of the annual percentage increases, includes the years 1964, 1965 and 1966![1] Now

[1] See his Table 3, but note that the time period referred to is deceptive. Albritton's data on caseload changes were taken from our tables, all of which end with February 1969. Throughout his critique, both

we certainly thought riots had an impact but, just as certainly, not retroactively. If Albritton had looked up the welfare increases after 1968, he would have found (as we reported in the epilogue to *Regulating the Poor*) that *the greatest period of rioting (1967–68) was followed by the greatest period of welfare growth (1969–70)*. There were 1.5 million families on the AFDC rolls at the end of 1968; by the end of 1970, another million had been added, for an increase of 67 percent in just two years (compared with 107 percent in all of the preceding eight years). Little wonder, then, that while his correlation moved in the right direction, it failed to reach statistical significance.

There is still another large error that depresses Albritton's correlation, and it arises from his superficial understanding of the welfare system. When he computed caseload changes, he was deceived by a welfare system sleight-of-hand in the reporting of caseload statistics; as a result he included some false data which greatly reduce the possibility of a correlation between rioting and welfare rises. Here we must pause to explain.

In 1961, Congress enacted an amendment to the Social Security Act making the two-parent family with an unemployed head eligible for AFDC benefits for the first time. About 67,000 families were admitted to the new AFDC-UP (unemployed parent) program in the 1961–64 period, and another 12,000 cases were added in the 1965–68 period. Albritton included both AFDC and AFDC-UP cases in his correlational analysis, and that was a serious mistake. For the fact is that the AFDC-UP cases did not consist of *new* families accepted for assistance, but of cases already enrolled in the General Assistance (GA) category which has traditionally contained a number of families with unemployed fathers. Since GA costs are borne entirely by states and localities, it was natural enough that the states would promote a wholesale transfer of these particular GA cases to the new AFDC-UP program in order to benefit from federal reimbursement. Thus 10 states opened AFDC-UP programs between April and December 1961. In April, their aggregate GA caseload stood at 212,000; by December, it had dropped to 160,300, a loss of 51,700, while the aggregate AFDC-UP caseload rose to 48,044. Our subsequent interviews with welfare directors confirmed that these cases had simply been transferred from the one category to the other. AFDC-UP cases were not, in other words, a genuine part of the great expansion of the AFDC caseloads which characterized the 1960s. For technical reasons, we included AFDC-UP cases in our tables in *Regulating the Poor*, but we repeatedly warned the reader that by doing so we had greatly "understated the difference in caseload growth between the earlier and latter parts of the decade" (1971, p. 334). Albritton should have paid attention to this and similar caveats for a reason that is simple to see. In the 1960–64 period, AFDC-UP cases represented 29 percent of the combined AFDC and AFDC-UP rise, thus greatly inflating the 1964 caseload which served as the denominator for calculating subsequent percentage increases in those places which implemented AFDC-UP.

Now with this point made, consider two additional facts. AFDC-UP programs were mainly opened in the northern states (only 4 of 17 southern states did so).[2] Consequently, it was *in the North* that Albritton greatly underestimated the magnitude of the post-1964 caseload rises. Moreover, it was *in the North* that severe rioting was more pervasive (southern cities experienced almost no conflagrations like those which erupted in Watts, Detroit, Newark and many other northern cities). Now, linking this chain of points together, we may see the second general way in which Albritton's correlationsl procedures are defective. By failing to exclude AFDC-UP cases, the post-1964 caseload percentage increases were *deflated* in the North where rioting was more widespread, relative to the South where rioting was less widespread. His correlation, in short, was depressed by the strong positive relationship between regional variations in both serious rioting and AFDC-UP implementation.

We hope Albritton will perform his correlations again, using appropriate post-rioting caseload measures, and with AFDC-UP cases excluded. Even with the depressing effects of both errors at work, he obtained a correlation which moved in a direction consistent with our analysis. With the errors corrected, the correlations could turn out to be statistically significant.[3]

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text and tables, he consistently refers to 1969 caseloads, as if for the full year.

[2] As to why almost all southern states refused to implement the AFDC-UP program, see *Regulating the Poor*, pp. 132–33.

[3] Albritton does not cite an earlier study which bears on this matter. Writing in *Social Problems* (1974), Michael Betz concludes that there was a strong relationship between rioting and enlarging state and local general assistance expenditures.

What, then, of crime and welfare? (As britton says, the impact of crime on welfare anges is "hardly a definitive test" of *Regulat-* *g the Poor,* but he investigated the matter yway.) In Tables 1 and 2, the errors of data anagement are as serious as in the riot rrelation tables. We spoke of changes in *black* ime rates; he averaged black *and* white rates. e measure of *post*-crime caseload changes is adequate. If shifts in crime rates were to be lculated through 1969, then caseload changes r at least 1969 and 1970 should have been cluded (and, it will be recalled, these were the o years of the greatest proportional caseload anges in the entire decade). To make matters orse, AFDC-UP cases are included, an error hich is even more troublesome than in the rlier discussion of the riot correlations. Con- ler Table 1. It contains the ten urban counties hich led the nation in proportional caseload creases after 1964. Five are in the states of eorgia and Texas, neither of which imple- ented AFDC-UP, and the other five are rthern counties with AFDC-UP. In other ords, half of the counties in the correlation, of them northern, have greatly deflated st-1964 percent caseload changes.

Table 1 is absurdly misleading for an addi- onal reason. We made much of the fact that any of the southern urban counties success- lly resisted admitting virtually any families at to the AFDC rolls, despite the great eco- mic suffering produced by the postwar agri- ltural upheaval and despite the turmoil of the rly 1960s. Consequently, when more cases ere finally accepted after 1964, spectacular rcentage increases resulted even though the merical increases were miniscule. The five uthern counties on this list are of just this nd, but that is not evident because the table ly shows percent changes, not actual caseload anges. Thus Lubbock had a grand total of 0 AFDC cases in 1964; over the next four ars it added 300, producing a 300 percent ange which placed it at the very top of the t for the entire country. By contrast, New ork, with 81,000 cases in 1964, added 1,000 for an increase of only 137 percent, acing it at the very bottom of the list. milarly, with an addition of only 900 cases, istin achieved an increase of 180 percent; but ith 56,000 cases added, Los Angeles could ly show growth of 145 percent. Taken gether, the five southern counties on this list ntributed a mere 11,000 cases, or less than 3 rcent of the total urban county increase after 64, while the remaining five northern coun- s contributed 175,000 cases, or 46 percent. It was bad enough that we had to include these particular southern counties in our list of 121, but to place five of them in a table with an N of ten is ridiculous. To conclude from such a table that "the data do not lend support to the Piven and Cloward thesis" is to use statistics mindlessly.

We note in passing that these errors of data management also undermine Albritton's challenge to our assertion that the welfare explosion took place in the late 1960s. He says that 85 percent of the AFDC-UP increase occurred before 1965, and we have already said why he is wrong to attribute significance to that. He also notes that 38 of 121 urban counties (defined as counties with a central city of at least 100,000 in 1960) had larger percentage increases in the first period; again, with AFDC-UP cases removed, the picture changes radically. Moreover, when caseload changes for the full decade are tabulated (not just for the first eight years), it is clear that virtually all urban counties had much larger percentage increases after 1965. It was truly an explosion, which is just how we characterized it.

A Spurious Explanation of the Welfare Explosion

Albritton's attempt to show that the welfare explosion was not a post-1964 phenomenon is curious, since he himself attributes the great rise in the welfare rolls to the social security amendments enacted in 1965; had he succeeded, he would have discredited his own explanation. He argues that the 1965 amendments "extended benefits to millions of new recipients and incentives to states to admit them to the rolls." Once more, he reveals a superficial knowledge of the welfare system, for his illustrations do not bear significantly on the AFDC rolls. He says, for example, that states "were permitted to provide retroactive assistance to AFDC families with children over 18 years of age who were still attending school or with minor children earning income." These changes mainly affected welfare budgets, not caseloads. Previously, earnings by minors had simply been deducted from the family's monthly check, as was that portion credited to the child who became 18 years old. Only under exceptional circumstances (where the 18-year-old continuing in school had been the youngest or only child, or where the minor's earnings had tipped the family over the income eligibility line) would these factors have affected eligibility and thus caseload statistics. In any case, the earnings of minors were not often reported, and few 18-year-olds in welfare families continued in school.

It is the Medicaid legislation on which Albritton mainly hangs his explanation. He says it created a large incentive for welfare directors to promote caseload growth because of a provision establishing a floor under the new federal medicaid reimbursements equal to 105 percent of the pre-existing medical assistance program. The notion that this provision generated a scramble on the part of welfare officials to enlist people onto the welfare rolls so as to raise the medical assistance costs on which Medicaid floors would be calculated is astonishing, both on logical and empirical grounds. It overlooks the enormous state and local costs such a response would have entailed in return for a slightly raised floor under federal Medicaid payments, since state and local governments had to bear as much as half of both the welfare and Medicaid costs they were presumably hastening to enlarge, no matter what the federal floor. And even if such a large disincentive did not cancel out a small incentive, the rolls would have been affected only in the period immediately after the amendments were passed. In fact, the AFDC rolls rose each year, at faster and faster rates, until the early 1970s.

Moreover, it would have been political suicide for welfare directors to set out to enlarge the rolls, and Albritton does not identify a single administrative mechanism by which caseload growth was deliberately spurred. In principle, intake workers might have been directed to make more permissive eligibility decisions, thus raising acceptance rates; or "out-reach" programs might have been developed to recruit the eligible but unaided, thus enlarging the volume of applications. Albritton does not intimate that either course was pursued, nor do we know of any instances, and so we are left with a not only illogical but a mystical connection between the Medicaid program and caseload growth. Just to be sure, we questioned Mitchell I. Ginsberg, who was New York City's commissioner of welfare in these years, about whether Medicaid provided incentives to welfare directors to bring new families onto the AFDC rolls. He just snorted. In the years after 1965, most welfare directors were progressively more preoccupied with how to *stem* welfare growth, for the fiscal repercussions of the growth were everywhere producing political headaches for local welfare officials. Moreover, the huge Medicaid costs imposed on states and localities greatly added to these fiscal and political travails (which are now no small factor in the Proposition 13 movement).

It is true, however, that Medicaid generated an incentive for officials in *other* institutions to promote welfare growth. Here Albritton notes that hospitals and health agencies referred indigent patients to welfare for Medicaid cards and many of these patients were then found to be eligible for welfare benefits as well. But the particular people to whom Albritton refers are "indigent patients in mental hospitals and tuberculosis institutions." Now we wrote *Regulating the Poor* about the explosion in the AFDC rolls, not about the rises in the categorical assistance programs for the aged and disabled (which, in any case, were dwarfed by the AFDC increases in this period). Before undertaking a critique, it would have been helpful if Albritton had got straight which categorical assistance programs he was talking about, and how Medicaid differentially affected them.

Finally, Albritton not only oversimplified our argument, he also trivialized it. In *Regulating the Poor,* we attempted to understand why public relief-giving institutions emerged in Western countries with the decay of feudalism and the growth of market economics. Further, we attempted to understand why these new institutions periodically expanded and contracted, why the proportion of the poor absorbed by them periodically rose and fell. The problem was to probe beneath the expressed motives of particular actors in order to identify more general socioeconomic and political regularities, whether in explanation of the relief explosion in the England of the 1830s or in the United States of the 1930s. At the most general level, the crucial combination of forces appeared to be these: mass unemployment, mass volatility, and vulnerable regimes. In attributing welfare expansion in the 1960s to the Medicaid legislation and other social security amendments, Albritton had an obligation to say how this explanation exemplified, at a more general level, the working-out of large-scale forces that were either similar to or different from those we had identified as being at the heart of relief explosions for nearly four centuries. That Albritton failed to grasp this is evident in his single sentence accounting for the enactment of the Medicaid legislation: "It is neither coincidental nor inconsequential . . . that this legislation was a product of the 89th Congress . . . a Congress determined to advance the interests of the poor" (p. 1010). We attempted to account for the sources of this rather unusual congressional determination in large-scale forces that were emerging in the society to which national electoral leaders were vulnerable. Albritton has no explanation, and does not seem to sense the need for one. With this statement, he dropped the matter. In a word, Albritton brought neither historical nor theoretical perspective to his critique. He not only trivialized *Regulating the Poor,* he also trivialized the question of why the poor sometimes win something from their rulers.

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A Reply to Piven and Cloward

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Piven and Cloward correctly believe that I was "led astray by [their] emphasis on the historic role of political disorder in producing relief liberalization" (p. 1014). I mistakenly read their work as an effort to propose a theory explaining historic expansions of welfare relief. Since the "War on Poverty" effort cannot be generalized to other historic periods, I regarded their discussion of this effort as a specific, but unique, mechanism (other historic periods produce their own mechanisms, usually different in structure) for translating social and political forces into public policy. I thought they were positing an interesting and significant theory of welfare policy, a theory focusing on "civil disorder, welling up in the context of electoral instability" (where the "context of electoral instability" would be a constant) to explain expansions of welfare caseloads. If Piven and Cloward see no direct causal impacts of civil disorders, i.e., riots, on welfare caseloads during the 1960s, I can accept this only as a major qualification of their work.

Perhaps I was "led astray" because so much of their analysis argues both implicitly and explicitly the contrary position that mass insurgency during the 1960s produced a rise in welfare caseloads of such proportions as to warrant characterization as a "welfare explosion." Despite their denials, Piven and Cloward claim that civil disorders, i.e., riots, *directly* contributed to expansion of the numbers of families receiving assistance from the Aid to Families with Dependent Children program and that the riots of the 1960s were partly a result of "the rate of increase of the black population" (1971, p. 239). It was these claims and only these claims that I examined in "Social Amelioration through Mass Insurgency. . . ."

The Relationship between Riots and Welfare

In *Regulating the Poor*, Piven and Cloward state:

> In the chapters to follow, we shall argue, as we did in our discussion of the relief explosion of the Great Depression, that the contemporary relief explosion was a response to the civil disorder caused by rapid economic change (1971, p. 196).

This is only one of their more explicit, unqualified assertions that a direct relationship exists between civil disorders and the level of welfare dependency. One can only wonder at the logic which suggests that statements such as the following: "economic convulsions which also produce mass turbulence—whether riots in the streets or upheavals in electoral alignments—are likely to lead to the temporary liberalization of relief provisions" (1971, p. 197) imply "interactive relationships" between civil disorders and electoral instabilities or an "indirect" impact of these factors on welfare. By linking riots and electoral instabilities with the word "or" rather than "and," their theory appears to hold that these are independent causal factors, not "necessary" and "sufficient" ones as they argue in their response. If they argue that electoral instabilities provide the "context" of the 1960s (although the degree of electoral support for the Democratic party in 1964 is not a prima facie example of electoral instability), it is not clear why this "context" should not be treated as a constant for purposes of comparative analysis, in which case the crucial issue is the impact of the presence or absence of civil disorder (the variable) on expansion of the welfare system during this period.

Now, Piven and Cloward also claim that only an indirect relationship exists between mass insurgency/unstable voting and welfare caseloads; that separate sets of relationships must be examined—first, the relationship between civil disorder and/or electoral instabilities) and legislative or administrative initiatives, and second, whether these legislative or administrative initiatives added to the welfare caseloads. This conceptual scheme is more consistent with my emphasis on impacts of specific legislation (which includes major amendments to the Social Security Act dealing directly with the welfare system, but completely missed by Piven and Cloward) enacted substantially *before* the period of the most intense riots, including the War on Poverty legislation. The time sequence ilitates against the Piven and Cloward view. s riots and other social disorders increased uring the 1966–69 period, ameliorative legis- ation was clearly on the wane. The data may e even more consistent with an inverse rela- ionship—as riots increased, ameliorative social egislation declined; or, conversely, as ameliora- ive social legislation declined, riots increased. y own view is that as civil rights protests ncreased during the early 1960s, ameliorative ocial legislation increased, culminating in the ignificant legislation adopted and implemented uring the 1964–66 period. As major rioting ncreased during the late 1960s, however, ameli- rative social legislation declined. Civil rights rotests and riots were perceived very different- y, something Piven and Cloward do not seem o comprehend.

Although Piven and Cloward explicitly deny ny attribution on their part of a direct elationship between riots and welfare, they ontinue to affirm such direct relationships ven in their response. How else can one read uch statements as:

> In turn, the more liberal dispensation of relief contributed to the ebbing of civil disorder in the ghettos (p. 1013).
>
> *the greatest period of rioting* (1967–68) *was followed by the greatest period of welfare growth* (1969–70) (p. 1016).
>
> Even with the depressing effects of both errors at work, he obtained a correlation (sic) which moved in a direction consistent with our analysis (p. 1016).

r, in footnote 3:

> Michael Betz concludes that there was a strong relationship between rioting and changes in state and local money payments for General Assistance. Of course, rising money payments mean rising rolls.

The Betz article (1974) contains so many ethodological and interpretive problems that t was too difficult to treat along with the ideas f Piven and Cloward in my original analysis. In act, I read the article as doing considerable amage to Piven's and Cloward's claims (al- hough this is not Betz's intention). The data n welfare spending is not state and local ayments for General Assistance as Piven and loward say, but all combined expenditures for elfare by state and local governments. Furthermore, Betz's data show that, in a comparison between riot and non-riot cities, expenditures for welfare increased by larger amounts in ities experiencing minor or no civil disorders than in cities which experienced major riots, 1968–69. (In fact, the major riot cities experienced a net decline in welfare spending during this period. See Betz's Table 1, p. 351.) In another test, applying a one-year lag of effects, the change in welfare spending in the year following the riots differed significantly from changes in other years at a level of significance below the .05 level in only one of the three tests (1974, p. 352). One reason I omitted the Betz article from my analysis was that he claimed to support the Piven and Cloward thesis, but I could never see how the analysis led to that conclusion.

What interests me most about the Betz article is that his theoretical relationships are identical to those suggested in "Social Amelioration through Mass Insurgency. . ." to test the Piven and Cloward thesis. (Apparently, Betz read them the way I did.) Betz uses a comparative form of analysis, with cities as the unit of analysis (I have used urban counties to be consistent with Piven's and Cloward's data), to test *direct* effects of riots or other major civil disorders on expansions of the welfare system. Piven and Cloward cite the Betz article favorably, not because it uses any different theoretical relationships, but because they believe that his conclusions support a position very important to their thesis—that riots cause welfare increases.

The Validity of the Analysis

Space limitations preclude any detailed consideration of all the distortions and misstatements in the Piven and Cloward response. (For example, nowhere do I argue against the concept of a welfare explosion occurring during the 1960s. Nor do I find the "correlations" in Tables 3 and 5 to which they refer. All I can find are mean percentages and statistical significance tests.) In considering the suitability of the data for the analysis, however, one should note that the data that Piven and Cloward find so flawed are the data from Source Table 2 in their *Regulating the Poor* which they asked readers to accept as supporting their analysis.

In any case, I will rely on their own arguments to justify the data. They indicate, for example, that "an unknown part of the AFDC-UP rolls is composed of families who were transferred from AFDC rolls, or who could have been admitted to the AFDC rolls in the absence of the AFDC-UP category" (1971, n. 184), a factor mitigating, if not eliminating, any error caused by inclusion of AFDC-UP cases. Their suggestion that an analysis be performed excluding the AFDC-UP factor is clearly facetious, since the data on urban counties does not distinguish between these types of recipients (1971, n. 184). When all factors concerning the data are considered, only “a small part of the Northern increase is due to AFDC-UP” (1971, n. 184).

It is not entirely clear why Piven and Cloward object to including AFDC-UP cases in the data. These are, after all, welfare families from whatever source. While qualifying former General Assistance recipients for AFDC-UP would pose some problems for comparing pre-1965 with post-1965 caseloads, there is virtually no threat to a cross-sectional design comparing changes in caseloads solely from the post-1965 period.

The major problem with Piven's and Cloward's critique of the data is their inability to comprehend the logic of comparative analysis, especially the cross-sectional design. When percentage increases in welfare caseloads for riot and non-riot urban counties over a span of years, 1965 to 1969, are compared, the riots, especially those occurring during 1967–68, are treated as an intervening variable. The percentage increases in welfare caseloads during this period are simply not “pre-riot” measures. Perhaps the reader can make more sense than I can of the statement that my measure of caseload changes “defined as the average of the annual percentage increases, includes the years 1964, 1965, and 1966” (p. 1015). I can only invite the reader to consult Piven's and Cloward's Source Table 2 (1971), subtract 1964 caseload from 1969 caseload, and divide by 1964 caseload. The result will be a percentage figure reported in their column headed “Percent Change 1964–69,” the data used for the most crucial tests in “Social Amelioration through Mass Insurgency. . . .” Piven's and Cloward's characterization of the data is simply wrong.

The correlations of crime incidence/rates (not rates alone) increases with increases in welfare caseloads (Table 2) and black population increases (Table 4) in “Social Amelioration through Mass Insurgency. . . ,” while not decisive, are damaging to Piven's and Cloward's assumption that the breakdown of social institutions among the black population as a result of migration led to “an alarming increase in serious crimes, such as armed robbery and burglary” (1971, p. 226). These tables show that increases in black populations in urban counties had no association with increases in incidence or rates of armed robbery and burglary. Similarly, although the evidence is weaker, even if blacks were responsible for crime increases, this form of civil disorder had no impact on welfare caseloads.

Piven and Cloward now argue that th “*greatest period of rioting* (1967–68) *wa followed by the greatest period of welfar growth* (1969–70)” (p. 1016). This is a peculiar argument not only because they say the do not accept the notion of a direct relation ship between riots and welfare, but also becaus they effectively discounted it by positing a alternative hypothesis in *Regulating the Poor* “The stimulus for this new upsurge was, w believe, the Nixon Administration's anti-infla tion strategy which sharply increased unem ployment” (1971, p. 341). In this statement they suggest a plausible rival explanation of th data that must be controlled before any causa relationship can be asserted between rioting an welfare growth, 1969–70. I suggest a similarl plausible rival hypothesis that explains som part of the welfare increase subsequent t 1964, since I find no relationship between riot and welfare.

Amendments to the Social Security Act adopted in 1965 and omitted from Piven's an Cloward's analysis, increased federal assistanc to states that raised levels of eligibility, thereb qualifying a new stratum of low-income families for assistance. Also under these amendments, families which ordinarily would leav the rolls because the youngest child reached the age of 18 could remain recipients for several more years, sometimes until the child finished college. For the first time in history, states were able to require the federal government to pay medical care costs for persons in state or local mental health or tuberculosis institutions by qualifying them for Medicaid under MAFDC where families were involved. Further, as Piven and Cloward suggest, Medicaid provided incentives for a variety of other institutions to make sure that all eligible patients were appropriately enrolled under public welfare programs. Collectively, these amendments provided substantially extended coverage and incentives to add eligibles to the welfare rolls. Welfare rights movements and community action agencies certainly played a significant role in informing eligible persons of benefits to which they were entitled. However, contrary to Mr. Ginzberg and his associates in New York, I found many welfare caseworkers in Chicago during this period eager to extend benefits to eligible persons whenever possible, not from any sense of threat from welfare applicants, but because they believed that the law intended that eligible persons should be provided assistance, civil service offered them some protection from political retaliation, and because almost none of the AFDC expenditures are borne by the city of

Chicago. (They are borne almost solely by the State of Illinois.)

Conclusions

If I was "led astray" by Piven's and Cloward's emphasis on the "historic role of political disorder in producing relief liberalization" (p. 1014), they were responsible for leading me (and other scholars who read them the same way, including Michael Betz) thus astray. Social scientists should be always more interested in such relationships that have profound implications for social theory than in excellent, but otherwise essentially journalistic accounts of the War on Poverty. In "Social Amelioration through Mass Insurgency. . ." I examined two sets of relationships that were explicitly posited in *Regulating the Poor,* using essentially the data Piven and Cloward provided. These were that "the difference between [urban areas] that experienced riots and those that did not was the rate of increase of the black population" (1971, p. 239), and that "riots in the streets . . . are likely to lead to the temporary liberalization of relief provisions" (1971, p. 197). The analysis, based on a cross-sectional design suggested by Piven and Cloward in the section "Local Responses to Disorder" (1971, p. 240 ff.) does not support these propositions.

I wonder if Piven and Cloward are prepared to explore fully the implications of the notion that there is no *direct* link between mass insurgency and welfare. Sometimes they seem to mean that civil disorder may or may not produce legislative or administrative responses, that these responses may or may not have some impact on ameliorating systems of social injustice. The evidence that it does is certainly thin. A "turbulent poor sometimes get something?" Like welfare? Well, maybe so, maybe not. In any case, mass insurgency is a highly dubious strategy for middle-class academics to urge upon the poor.

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Ethnicity and Socioeconomic Status as Sources of Participation: The Case for Ethnic Political Culture

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Many studies undertaken in recent decades have documented the effects of socioeconomic status (SES) on political participation. As consensus has grown on the role of socioeconomic status, other factors, like ethnicity, have been relegated to secondary importance. However, variations in levels of participation can be traced, in part, to differences in ethnic political culture. Furthermore, the findings presented in this article indicate that holding socioeconomic status constant does not eliminate the independent impact of ethnicity on political attitudes that affect participation. Ethnicity is shown to have a greater effect than socioeconomic status on levels of participant political culture. How these findings might influence our understanding of social and political inequality is discussed, and students of participation are urged to give more serious attention to the ethnic factor.

For several decades a strong consensus has been building on the underlying sources of political participation. Increasingly, socioeconomic status is being viewed as the single most important social background influence on levels of participation behavior. Verba and Nie (1972) aptly symbolize this consensus when they refer to the relationship between socioeconomic status and participation as the "Standard SES Model." As consensus has grown on the role of socioeconomic status, attention has been diverted from the influence of other social background factors like personality and ethnicity. Verba and Nie, for example, in their already classic work on participation, make only one passing reference to ethnicity and no reference to personality per se. When compared to earlier works on participation by such writers as Lane (1959), Milbrath (1965), and Berelson, Lazarsfeld and McPhee (1954), the omission is striking and indicative of a trend that plays down the importance of non-socioeconomic factors.

There are two major reasons why ethnicity has been relegated to secondary status as an explanation of American political participation. First, many social scientists have been so steeped in the normative dimensions of the "melting pot" myth that they are unwilling (or unable) to conceive of ethnicity as an enduring and important element of social division. Certainly race has long been recognized as a lasting basis of socio-political conflict, but the belief that religious and nationality aspects of ethnicity could also withstand the onslaught of the "inevitable" assimilation process is recent.

The second reason (related to the first) centers on the empirical relationship between ethnicity and social class.[1] It is not difficult to distinguish conceptually between social class and ethnic status. In the United States, at any rate, class status is based primarily on the achievement criteria of income, educational level, and occupational prestige, whereas ethnic status is derived from such ascriptive criteria as race, religion, and nationality.[2] But however clear the conceptual distinctions between ethnic and class status, they are nonetheless highly related empirically. Those who share a common ethnic background also tend to share a common

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[1] Researchers commonly employ measures of socioeconomic status as surrogates for the broader, richer concept of "objective social class." The terms "socioeconomic status" and "social class" are used interchangeably throughout this paper.

[2] The concepts of achievement and ascription were, of course, first developed by Talcott Parsons to compare traditional and modern societies. See his work, *The Social System* (1951), for a full description of "pattern variables," including the "achievement-ascription" dimension of societies. Although Parsons uses this pattern variable to compare societies, it is also useful for conceptual distinctions between different forms of social stratification.

class position. And given the high empirical correlation between class and ethnicity, it is difficult to determine whether each has an independent influence on participation. Simply put, the question is as follows: does one individual participate more than (or in different ways from) another because of ethnicity, or because of social class position, or both?

Despite these three possible answers to the question, only the latter two arguments are given much credence in the literature on participation. Many contend that the relationship between ethnicity and political participation is spurious, and that social class is the "true" causal factor. Implicitly, the argument is based on the notion that because socioeconomic status is related to participation in non-ethnic populations, and because ethnic groups can be distinguished one from another along socioeconomic lines, differences in the level of participation between ethnic groups are merely the result of their socioeconomic differences. Milbrath (1965, p. 138) makes perhaps one of the more extreme statements of this position when he claims: "Physiological racial characteristics do not account for participation differences between races; rather, it is the relative social position of racial groupings that create these differences." That there might be non-class differences between races that account, in part, for differences in participation is not acknowledged by Milbrath.

There is also a growing but less prominent body of literature arguing for the joint impact of ethnicity and social class. Those taking this view (implicitly or explicitly) rely on the notion of "ethclass" first developed by Milton M. Gordon (1964). That ethnicity may have an impact independent of its relationship to social class is a position held by relatively few students of participation. One consequence of these views is the paucity of empirical studies of participation which focus on ethnicity. Theories relating ethnicity to political participation are equally rare, and those that have been put forward are not very advanced.

The goal of this article is to develop and test a political culture theory to explain ethnic differences in levels of political participation. I shall focus on non-electoral behavior, where the intention of the participant is clearly to influence the distribution of resources by local political authorities. I will label such participation "communal," and will examine four forms: contacting local public officials about community problems, joining community problem-solving organizations, signing petitions for neighborhood improvement, and attending community political protest demonstrations.

To examine the political attitudes and participation patterns of American blacks, Cubans, Dominicans, Irish, Jews, and Puerto Ricans, I will use data based on survey research conducted in northern Manhattan during 1973. Because the conclusions of this study are based on survey data from only one location (i.e., New York City), a note of caution is in order about how far the findings are generally applicable. Even the most casual visitor to New York is aware of its profoundly ethnic character. Thus, a case could be made that the salience of ethnicity to politics is greater in New York than in most or all other major American cities.

New York, however, is not unique in the fact that its citizens are divided along ethnic lines. Nor are the findings of this study about the effects of ethnicity on political attitudes unique. Using national-level data, Greeley (1974) found strong evidence to support the thesis that ethnicity is a major determinant of political attitudes and participation in America. Lenski (1961) and Laumann (1973) report findings based on survey data from Detroit that are consistent with the thesis of this study, as do Antunes and Gaitz (1975) in their analysis of white, black and hispanic participation in Houston. In short, studies in other contexts cast doubt on the notion that the findings of the present study are applicable only to New York City.

The first section of this article explores the meaning of culture and how cultural differences can affect political participation. A case is made for linking together culture and ethnicity in an explanatory model of participation. Later sections employ multivariate analysis to test the validity of the proposed ethnic cultural model. I will explore the relationship between socioeconomic status and political culture, since the "Standard SES Model" can be viewed as the main alternative thesis to my own ethnic cultural argument. In addition, two other alternative explanations will be examined. The first I will label the *assimilationist* thesis, which maintains that some ethnic groups participate in politics more than others simply because they are more assimilated into mainstream American society. The second, which I will call the *ethnic political consciousness* thesis, contends that ethnic group differences in participation "wither away" when individuals from the least politically active groups gain a strong identification with the political goals and interests of their ethnic groups. The concluding section of the article examines the findings in relation to both the "Standard SES Model" and the issue of ethnic inequality in America.

Culture, Problem Solving, and Political Participation

While the concept of *culture* is central to anthropology, the use of the term has not by any means been restricted to that discipline; rather, it has been used by economists, sociologists, psychologists, social psychologists, and political scientists as well. The concept of culture, then, ranks with *class, status, social system, power,* and *personality* as one of the basic concepts of the social sciences.

There are many definitions of the complex term, "culture," some of them contradictory. Some definitions are broad and seem to include all forms of social behavior and psychological states, as well as the material products of society like art, literature, food, and music. Other definitions are more restricted.

However, most definitions agree that culture is a shared set of values, norms, beliefs, and attitudes. Furthermore, many definitions stress the relationship between culture and *problem-solving* behavior. Some have even defined culture exclusively in such terms. Ford, for instance, states that culture "consists of learned problem-solutions" (1963, p. 106). Young equates "folkways" with culture and defines folkways as "continuous methods of handling problems and social situations" (1963, p. 106). Oscar Lewis (1966, p. 18), in a discussion of the function of culture for the individual, points out that culture "provides human beings with a design for living, with a ready-made set of solutions for human problems so that individuals in each generation do not have to begin all over again from scratch."

Kluckhohn and Strodtbeck, using the conception of culture as a problem-solving mechanism, have attempted to specify the ways in which culture affects social behavior. They argue that the ethos (culture) of a people is "indispensable to the interpretation of concrete behavior" (1961, p. 1). To them, the best indicators of ethos are value orientations, which are essential for interpreting social behavior because they "give order and direction to . . . human acts and thoughts as these relate to the solution of 'common human' problems" (p. 4). Kluckhohn and Strodtbeck, then, bring two common elements of definitions of culture together. They maintain that the most basic value orientations of cultural groupings are those that guide individual problem-solving behavior.

If culture provides guidelines for problem-solving behavior in general, then it follows that *political* culture acts as a guide to problem solving in the political realm. As Verba puts it, political culture affects "when and in what ways individuals become involved in political life" (Pye and Verba, 1965, p. 517). Such involvement in politics is often expressed through various forms of political participation. Although some forms (like voting) do not always have a clear-cut problem-solving base, many kinds of participation are essentially motivated by a desire to solve some problem or set of problems. This is especially the case when individuals (alone or with others) contact political authorities or agencies. The problem being addressed is the absence or relative scarcity of some resource (e.g., money, job security, respect, influence, etc.) that people believe can be gained through political participation. Contacting public officials, signing petitions, joining certain types of political organizations, and attending protest demonstrations are all forms of participation that are based on problem-solving motivations.

Since political culture consists of those value orientations which guide people to solutions in the political realm, forms of political behavior that are rather straightforward expressions of problem-solving action should be highly related to political culture. Some people will participate more than others, in part because they exhibit a political culture more supportive of participation—i.e., what might be labeled "participant political culture."[3]

Given the salience of participant political culture as a source of participation behavior, one important task is to establish the determinants of such culture—that is, what explains variations between people in levels of participant culture? Much previous research has identified socioeconomic status as a key factor.[4] While I make no attempt to take issue with the findings of this research, my thesis here is that the impact of ethnicity on participant culture is at least as important as that of socioeconomic status, and may be more important. It can be openly questioned whether the dual processes of acculturation and assimilation have so eroded the cultural bases of ethnic groups that stinctive ethnic political styles are no longer entifiable. Fundamental value orientations die rd, and unless assimilation runs its full urse, distinctive ethnic value orientations will main to act as guides to distinctive ethnic cial and political behavior.

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[3] Gabriel A. Almond and Sidney Verba (1965) coined the term "participant political culture" to apply to the political cultures of nations. There is no reason, however, why the concept cannot also be applied to individuals and groups.

[4] While Verba and Nie (1972) is probably the best current statement of the SES thesis, a vast literature exists linking socioeconomic status to levels of political participation. For a good summary of the major works on this subject, see also Milbrath and Goel (1977), especially Ch. 4.

Measurement of Political Culture

How, then, is political culture to be mea- red? Triandis (1972, p. 80) points out that lue structures are best reflected in attitudes. us, to measure political culture, it is neces- ry to identify basic political attitudes. Of the titudes Verba identifies as basic to political lture, four stand out as pertinent to the oblem-solving framework of the present stu- : *involvement, efficacy, trust,* and *civic areness* (Pye and Verba, 1965, pp. 537–42). fifth attitude, *community political knowl- ge,* is also relevant to an examination of litical culture and participation in a local tting.

Political involvement is an attitude which volves a generalized interest in politics, a ndency to discuss politics with others, and an derstanding of political issues. In a problem- lving framework, it reflects the general value individual places on the potential effec- eness of political solutions in general. There- re, when individuals exhibit a high level of litical involvement, we can assume that they ill be predisposed to political action, since the titude itself reflects the positive value they ace on political action.

If political involvement reflects the indivi- al's value orientation to the political system, e *sense of political efficacy* reflects one's ews of one's own role in that system. When dividuals exhibit a high sense of political ficacy, they place a high value on their worth the political arena. Political efficacy, then, n be defined as a belief in one's ability to fluence political authorities and other politi- l actors. A high sense of political efficacy will nd to encourage participation, because others e perceived to be responsive to the individu- 's demands. Conversely, we expect a low sense efficacy to inhibit political participation.

Like political involvement, *political trust* flects a value orientation toward the political stem. Like the sense of efficacy, trust is also evaluative dimension of political attitudes d measures the degree to which public ficials are esteemed. In a problem-solving ntext, political trust can be defined as the lue people place on political authorities as potential problem solvers. Political trust is a complex attitude that includes an evaluation of the moral character of public officials, their competency, and their responsiveness to people. Individuals who exhibit high levels of trust will tend to be *non*-participants, since they assume it is safe to leave the solution of problems in the hands of trusted public officials. They feel little need to influence decision makers, who are seen as acting on behalf of the general welfare of the community. *Political cynicism,* or low political trust, is therefore presumed to be a source of participation, since it reflects the belief that officials cannot be trusted to act in the best interests of the community without pressure to do so.

Civic awareness usually reflects the value one places on the general welfare of the community. Specifically, an individual who knows about community problems, and is also aware of which individuals, groups, or organizations are attempting to solve these problems, is exhibiting civic awareness. It can be hypothesized that those exhibiting high levels of civic awareness will be more prone to seek political solutions to community problems than individuals who are unaware of the nature and severity of such problems or the actions being taken by others to solve them.

Community political knowledge means the individual's knowledge of people and places in the community that help people deal with local government. It is related to both political involvement and civic awareness, in that it reflects a high value placed on seeking political solutions and, to some extent, an awareness of what is going on in the community. Those with high levels of community political knowledge will tend to take political action because they are aware of concrete places and people who are likely to respond to their initiatives. Conversely, those who are otherwise politically inclined may not seek political solutions to their problems when they lack knowledge about where to go for help.

Operational Definitions

I have standardized and combined three survey questions to form a "political involvement index." The questions were as follows: (1) "Some people don't pay much attention to politics. How about you? Would you say that you are very much interested, somewhat interested, not very interested, or not interested at all?" (2) "And what about national political questions? Do you talk about national political questions with people you know well—often, sometimes, rarely, or never?"[5] (3) "How about local issues in New York City? Would you say that you understand them very well, pretty well, not too well, or not at all?"

The "political efficacy index" was formed from two interrelated questions: "Suppose a law were being considered by the New York City government that you considered unjust or harmful. What do you think you could do? If such a case arose, how likely is it that you would actually do something about it—very likely, pretty likely, not too likely, not likely at all?" Open-ended responses to the question "What do you think you could do?" were dichotomized into "could do something" and "could do nothing." I then standardized and combined the two variables to form the overall index.

The "political cynicism index" was comprised of the following questions: (1) "How much do you think you can trust the government in New York City to do what is right—just about always, most of the time, some of the time, or almost never?" (2) "Would you say that the government in New York City is run for the benefit of a few big interests or that it is run for the benefit of all people?" and (3) "How about New York government officials—do you feel they generally do their jobs well c don't they—just your impression?" Negative responses were assigned high response code numbers for each question. I standardized an combined the variables to form the overa index.

The "civic awareness index" was comprise of three questions: (1) "How effective woul you say local organizations are in dealing wit neighborhood problems?" (2) "Have you hear or read anything about the Neighborhoo Action Program for this part of the city? Ca you remember anything they have been doing?" and (3) "Is there a local (neighborhood newspaper that carries news about your area What is the name of the paper?" Because of th large number of "don't know" responses to th first question, I divided responses into "know about community organizations" and "does no know about community organizations." coded the other two questions for their cognitive content; that is, if respondents could nam an activity of the Neighborhood Action Program they were coded as "yes," if they coul name a local (neighborhood) newspaper the were coded as "correct." The overall index wa a simple additive one composed of the thre dichotomized variables.

"Community political knowledge" was single variable based on the following question "Are there any places, or groups, or particula people in this neighborhood who help peopl deal with city offices and services? What ar their names?" The resultant variable was dichotomous—those who could name a person, place or organization helping people deal with loca government were coded "yes," those who coul not were coded "no."

The five political attitudes described abov were all used as indicators of participant political culture. Further, because the concept o culture generally refers to a set or cluster o values, norms, beliefs, and attitudes, we ca anticipate that the five attitudes selected fo analysis will be highly related empirically. Tabl 1, a Pearson *r* (zero-order) correlation matrix

Table 1. Pearson *r* Zero-Order Correlations between Political Attitudes (N=379)

Attitude	1	2	3	4	5
1. Involvement	–	.48	.38	.32	.22
2. Efficacy		–	.26	.19	.17
3. Awareness			–	.40	.23
4. Knowledge				–	.15
5. Cynicism					–

Source: Ethnic Block Survey, New York City Neighborhood Project, Bureau of Applied Social Research, Columbia University, 1973.

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[5] The survey instrument did contain a question on discussion of *local* politics with friends. It was found to be highly correlated with the other two items of the "political involvement index," but not as highly as discussion of *national* political questions. Since the intent was to measure a general political involvement orientation, discussion of national political questions was selected for the index.

Technically, discussion of politics is a *behavioral* rather than an attitudinal measure. However, it was included as an indicator of political involvement because it is not clearly a form of instrumental behavior (like contacting public officials, for example). That is, the mere discussion of politics with friends and/or acquaintances need not lead to overt problem-solving action. Discussion of politics is better viewed as an expression of conscious interest in politics.

explores the assumption that the five political attitudes are intercorrelated.

The findings in Table 1 provide convincing evidence that the attitudes chosen to measure participant culture are strongly related. Each attitude exhibits a positive correlation with the other four attitudes. On the basis of the correlation matrix, I standardized and combined the five attitudes to form an overall attitude index.[6]

Identifying and defining relevant political culture concepts has laid the necessary groundwork for testing the thesis of this study. The section that follows begins with an examination of political problem-solving behavior identified above as communal participation. I used factor analysis to explore the dimensionality of participation variables, and to demonstrate the distinctiveness of communal items from other forms of participation. Next I examined the relationship between communal participation and participant culture, to test the proposition that political attitudes selected for analysis do in fact tend to predispose individuals to become communally active.

Communal Participation and Participant Culture

A number of survey questions were used to determine whether the respondent had in the past few years contacted a New York City official or agency or performed other political problem-solving acts. The seven acts identified by the questionnaire were as follows: (1) contacting (i.e., calling, writing, or visiting) a city official or agency about neighborhood problems; (2) attending a community protest meeting, joining a protest parade, or picketing about neighborhood problems; (3) signing a petition about neighborhood problems; (4) joining a community organization to deal with neighborhood problems; (5) voting in a citywide election; (6) individually contacting an unemployment, social security, or welfare office for help with a problem related to financial subsistence; and (7) contacting a city official or agency to seek personal (particularized) services. Table 2 reports the proportion of respondents performing each act.

With the aid of factor analysis I attempted to determine whether the four *communal* participation acts (contacting officials, joining organizations, signing petitions, and attending protest demonstrations) constitute a form of political behavior distinct from voting, subsistence political activity, and personal service seeking. Table 3 presents the factor loadings (without rotation) from a principal factor analysis (PA2) of the seven kinds of political activity. I included only U.S. citizens in the analysis, since voting and subsistence political activity require such citizenship.

The data in Table 3 are quite revealing as to the nature of political participation at the local level. The table demonstrates that there are four distinct dimensions of political activity, three of which are interrelated. Subsistence activity is shown to constitute one dimension by itself. It is uncorrelated with the communal items and has a moderately negative correlation with personal service seeking (component II). Voting, while more highly correlated with communal activity, has too low a factor loading to be considered a communal act. Seeking

Table 2. Percent of Respondents Performing Political Problem-Solving Acts (N=379)

Problem-Solving Act	Percent	No.
Voting	52.8	(200)
Signing Petitions	39.6	(150)
Seeking Subsistence Aid	34.6	(131)
Seeking Personal Services	20.1	(76)
Contacting Officials About Community Problems	15.0	(57)
Joining Community Organizations	13.2	(50)
Attending Community Protests	7.7	(29)

Source: Ethnic Block Survey, New York City Neighborhood Project, Bureau of Applied Social Research, Columbia University, 1973.

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[6] A principal factor analysis (PA2), without rotation, was also performed using these five attitudes. Only one factor resulted. Significant loadings (i.e., greater than .50) existed for all but political cynicism, which had a loading of only .32. Its lower loading reflects the fact that cynicism does not correlate very well with efficacy and knowledge. However, because its correlation is high with political involvement and civic awareness, and because of the theoretical import of cynicism to political analysis in general, it was decided to include cynicism in the overall index of political attitudes.

Table 3. Principal Factoring with Iteration (PA2): Unrotated Factor Matrix for Seven Political Acts among Citizens Only (N=275)

Political Act	Component I	Component II	Communalities
Joining Organizations	.71	–.25	.56
Contacting Officials	.60	.04	.36
Signing Petitions	.51	.07	.26
Attending Protests	.48	.04	.23
Voting in City Elections	.28	.04	.08
Personal Service Seeking	.20	.56	.36
Subsistence Activity	–.09	–.19	.05
Percent of Total Variance	77.4	22.6	–
Eigenvalue	1.47	0.43	–

Source: Ethnic Block Survey, New York City Neighborhood Project, Bureau of Applied Social Research, Columbia University, 1973.

personal services (particularized contacting) has a weak positive loading (.20) on component I, but component II explains a much higher level of its variance (i.e., 31.6 percent); it is therefore best viewed as a separate dimension of participation.

Subsistence political activity is essentially unrelated to all three dimensions of participation. It does not exhibit the "voluntarism" characteristic of the other three items, and is therefore best conceived as "non-participatory" political activity. That service seeking, voting, and communal participation are intercorrelated suggests that they are all measuring some form of participation. But the results of the factor analysis lead to the conclusion that it is better to construct three distinct measures of participation (communal, electoral, and service seeking) than to create a single participation index. Taking into account the size of factor loadings and correlation analysis, I combined the four communal items into a simple additive index that shall be called "communal participation."

Having established the distinctiveness of communal participation, next I explored its relationship with participant culture. Table 4 presents the Pearson *r* zero-order correlations between participant culture and communal participation items, reporting individual communal acts and political attitudes to give a more comprehensive picture of the relationship.

The importance of political attitudes to communal participation can be easily ascertained from Table 4. The relationship between the participant culture and communal participation indexes is particularly strong ($r = .49$). As a predictor variable, participant culture alone explains about 24 percent of the variance in participation. It is clear that any factor which significantly affects participant culture will also strongly influence communal participation, albeit indirectly. Others have demonstrated the central role that socioeconomic status plays in the formation of participant culture. In the data analysis to follow, multivariate techniques will be used to demonstrate that ethnicity also

Table 4. Pearson *r* Zero-Order Correlations between Political Attitudes and Communal Participation (N=379)

Attitude	Participation Index	Contact Officials	Join Organizations	Sign Petitions	Attend Protests
Involvement	.36	.30	.26	.26	.18
Awareness	.37	.28	.27	.28	.16
Efficacy	.33	.23	.25	.25	.17
Knowledge	.30	.34	.19	.19	.14
Cynicism	.24	.14	.16	.17	.22
Attitude Index	.49	.40	.35	.35	.27

Source: Ethnic Block Survey, New York City Neighborhood Project, Bureau of Applied Social Research, Columbia University, 1973.

has a powerful impact on participant culture, and that its role in the formation of attitudes is at least as strong as that of socioeconomic tatus.

The Effects of Ethnicity on Participant Culture

Ethnic groups have been defined alternaively as cultural groups, psychological groups, ocial groups, social class groups, status groups or as some combination of these. But at the ndividual level of analysis the important disinction to be made is between *psychological* and *nominal* identity. Psychological ethnic dentification is subjective, in the sense that ndividuals decide for themselves to which ethnic group they "belong," or for that matter whether they feel *any* attachment to their ethnic heritage. In the United States, at any ate, nominal identity is usually based on the more objective factors of race, religion, and nationality. For the purposes of this study, ndividuals will be classified as to their nominal ethnic identity. Since my goal is to examine political value orientations, and not the effects of ethnic consciousness on political behavior, he objectively based nominal identification of ethnicity appears to be more appropriate. References to ethnicity, then, shall mean nominal ethnic identity.

The survey question used to determine nominal ethnic identity is as follows: "Here is a ist of several groups. [The respondent was handed a sheet with ethnic group names, ncluding "other."] Which *one* group best describes the background of most of your ancestors?" The question clearly elicits an "objective" response, since the respondent is not asked how closely she/he identifies with an ethnic group.

How much impact does ethnicity have on participant culture? Put another way: to what extent does the knowledge of an individual's nominal ethnic identity enable one to predict his/her political attitudes? Table 5 begins answering this question by way of multiple classification analysis; its purpose is to determine how much variation in participant culture is "explained" by ethnic identity alone. Six ethnic groups will be analyzed: Irish, American blacks, Jews, Puerto Ricans, Cubans, and Dominicans.

The data reported in Table 5 provide strong initial support for the proposition that ethnicity is a major predictor of participant culture. With an eta^2 (or R^2) of .601 with the overall attitude index, it can be said that ethnic identity alone explains 36 percent of the variation in participant attitudes. And although the amount of variance explained by ethnicity does vary among individual items of the attitude index–from a low eta^2 (or R^2) of .098 to a high of .312–for all five items the correlation is substantial.

There is the possibility, however, that the relationship discovered between ethnicity and participant culture is spurious. Social class differences between ethnic groups are large, and are reflected in the rather high correlation between ethnicity and socioeconomic status (R = .48). Therefore, participant culture differences could be solely or primarily a function

Table 5. Multiple Classification Analysis (MCA): Political Attitudes with Ethnic Identity* (N=379)

Ethnic Identity	Attitude Index	Civic Awareness	Political Involvement	Community Political Knowledge	Political Cynicism	Sense of Political Efficacy
rish (n=58)	.72	.77	.49	.36	.22	.52
Black (n=63)	.58	.56	.24	.49	.40	.21
Jewish (n=69)	.38	.30	.42	.20	.24	.07
Puerto Rican (n=50)	–.10	–.31	.04	–.18	.22	–.10
Cuban (n=78)	–.58	–.57	–.34	–.38	–.47	–.15
Dominican (n=61)	–.89	–.67	–.79	–.44	–.46	–.52
**Eta=	.601	.559	.451	.362	.362	.313
**Eta^2=	.362	.312	.204	.131	.131	.098

Source: Ethnic Block Survey, New York City Neighborhood Project, Bureau of Applied Social Research, Columbia University, 1973.

*All attitudinal variables are standardized; thus, numbers reported in the table are plus/minus standard deviation units from the mean of zero.

**Eta and Eta^2 are equivalent to R and R^2 derived from multiple regression analysis.

of socioeconomic differences between ethnic groups. On the average, members of white ethnic groups like the Irish and Jews tend to be much older than blacks and hispanics residing in central city areas, so ethnic differences could also reflect the impact of age on participant culture. Further, men were inadvertently oversampled in some ethnic groups, and owing to traditional sex role norms, men may tend to exhibit higher levels of participant culture than women. Finally, ethnic groups also vary in terms of their average length of residence in the community. It is conceivable that the longer one resides in a community, the more knowledgeable one becomes about community political processes; hence, ethnic differences in participant culture could simply be a reflection of demographic factors like length of residence. More important, longer residency may reflect higher levels of assimilation and acculturation. Length of residence, then, may be a surrogate measure of cultural adaptation and may account for much of the difference in participant culture exhibited by ethnic groups in this study.

Table 6 is an attempt to test for possible spuriousness. Socioeconomic status,[7] age, sex, and length of residence[8] are introduced int multiple regression analysis along with ethnicity, to determine whether ethnic differences ar "real" (i.e., the result of ethnic factors) o simply by-products of other correlates of participant culture.

Table 6 provides interesting information which requires some interpretation. Both ethnicity and socioeconomic status prove to be th strongest predictors of participant culture Length of residence has a marginally importan impact (beta = .13), while sex and particularl age explain very little about political attitudes.[9]

Table 6. Multiple Regression: Political Attitudes with Ethnic Identity and Selected Control Variables (N=379)

Independent Variable	Pearson *r*	Beta
Socioeconomic Status	.49	.31**
Sex (Male +)	.19	.09*
Age	.01	–.01
Length of New York City Residence	.50	.13*
Ethnic Identity[a]	.60[b]	(.41)[c]**
Irish (n=58)	.13	.15**
Blacks (n=63)	.08	.18**
Puerto Ricans (n=50)	–.16	–.07
Cubans (n=78)	–.39	–.18**
Dominicans (n=61)	–.46	–.23**

Multiple R = .69
Multiple R^2 = .47

Source: Ethnic Block Survey, New York City Neighborhood Project, Bureau of Applied Social Research, Columbia University, 1973.

[a] Nominal ethnic identity is represented by dummy variables; Jews are the excluded (i.e., comparison) group.
[b] Coefficient is a Pearson R.
[c] Beta coefficient obtained from Multiple Classification Analysis (MCA).
$*p < .05$
$**p < .01$

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[7] The "index of socioeconomic status" is composed of three variables: educational achievement (number of school years completed), score on the National Opinion Research Center's occupational prestige scale, and family income before taxes. Variables were firs standardized and then combined to form the overal index.

[8] The variable "length of residence" was compute by dividing the number of years the respondent ha lived in New York City by his or her age. Such procedure was necessary because of the obviou correlation between age and length of residence. I effect, "length of residence" refers to the "percentag of one's life spent living in New York City."

[9] When sex and age were introduced last into th analysis via stepwise regression, they explained les than 1 percent additional variance in political attitudes. The relationship between age and politica attitudes was also examined for possible curvilinearity but the eta was found to be statistically insignificant Since the betas for sex and age suggest that thes variables add little to the explanation of politica

A comparison of unstandardized regression coefficients with their standard errors for all independent variables reveals that a strong probability that the betas are zero exists only for the variables age and Puerto Ricans. The fact that the multiple R^2 for the entire regression is .47 means that the independent variables explain almost half of the variance in participant culture.

A problem arises, however, in attempting to interpret the effects of ethnicity, socioeconomic status, and length of residence. The beta for each is substantially lower than its zero-order correlation.[10] Reductions in correlation reflect the fact that these three independent variables are highly intercorrelated: i.e., socioeconomic status with ethnicity, $R = .48$; socioeconomic status with length of residence, $r = .40$; and ethnicity with length of residence, $R = .69$. I therefore performed a commonality analysis to sort out the unique and common variances explained.[11] Table 7 reports the results.

The commonality analysis reveals much about the joint and separate impacts of ethnicity, socioeconomic status, and length of residence. Length of residence is shown to be a very weak predictor of participant culture. It uniquely explains only 0.7 percent of the variance in political attitudes, compared to 11.4 percent for ethnicity and 7.9 percent for socioeconomic status. Its effects are particularly related to ethnicity (i.e., length of residence and ethnicity share 10.4 percent of the explained variance); and the fact that a large portion of variance explained by ethnicity (i.e., 11.4 percent) is *not* shared with length of residence suggests a spurious direct relationship between length of residence and political atti-

Table 7. Commonality Analysis: Unique and Common Variances in Political Attitudes Explained by Ethnic Identity, Length of Residence and Socioeconomic Status (N=379)

Variance Explained	1 Ethnic Identity	2 Length of Residence	3 Socioeconomic Status
R^2 Unique to 1	.1141		
R^2 Unique to 2		.0070	
R^2 Unique to 3			.0792
R^2 Common to 1 and 2	.1040	.1040	
R^2 Common to 1 and 3	.0179		.0179
R^2 Common to 2 and 3		.0149	.0149
R^2 Common to 1, 2, and 3	.1255	.1255	.1255
Sum:	.3615	.2514	.2375
Multiple R^2 = .4626			

Source: Ethnic Block Survey, New York City Neighborhood Project, Bureau of Applied Social Research, Columbia University, 1973.

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attitudes, they can safely be dropped from the analysis that follows.

[10] Using multiple classification analysis I calculated a summary *partial* beta of .42 for ethnicity, controlling for socioeconomic status and length of residence. Given that the zero-order relationship between participant culture and ethnicity is .60, the effects of ethnicity on participant culture appear to be somewhat contaminated with socioeconomic status and length of residence.

[11] Commonality analysis, sometimes called elements analysis, is a method of decomposing the variance of a dependent variable into common and unique variances to allow the analyst to interpret the relative influences of n independent variables. In the three variable case, the following formula applies:

$$R^2y.123 = U(1)+U(2)+U(3)+C(12)+C(13)+C(23)+C(123).$$

For identifying each unique and common variance, the following formula applies:

$$U(1) = -R^2y.23 + R^2y.123$$
$$U(2) = -R^2y.13 + R^2y.123$$
$$U(3) = -R^2y.12 + R^2y.123$$
$$C(12) = -R^2y.3 + R^2y.13 + R^2y.23 - R^2y.123$$
$$C(13) = -R^2y.2 + R^2y.12 + R^2y.23 - R^2y.123$$
$$C(23) = -R^2y.1 + R^2y.12 + R^2y.13 - R^2y.123$$
$$C(123) = R^2y.1 + R^2y.2 - R^2y.3 - R^2y.13 - R^2y.23 + R^2y.123.$$

See Kerlinger and Pedhazur (1973, pp. 297–305) for a more complete discussion of commonality analysis.

tudes. Ethnicity and socioeconomic status, then, emerge as the key explanatory variables. Together they explain about 45.5 percent of the variance in participant culture. (See Table 8.) It is clear that eliminating age, sex, and length of residence from the explanatory model does little to diminish its predictive capability.

Results of the commonality analysis suggest the need for a closer examination of the separate and joint impacts of ethnicity and socioeconomic status. Which one has the greater impact on participant culture? How much variance in participant culture is uniquely explained by each? How much explained variance do ethnicity and socioeconomic status share in common? In order to answer these questions, I used commonality analysis again. Table 8 reports the results for the participant culture index and each of the five attitudinal items making up the index.

The results reported in Table 8 are startlingly clear regarding the joint and separate impacts of ethnicity and socioeconomic status. On the average, for all six items in the table, about 72 percent of the variance in participant culture explained by these two variables can be attributed to the *unique* contributions of each. Interaction effects (multiplicative terms for each ethnic group and socioeconomic status) were examined and found to be statistically insignificant. Thus, although ethnicity and socioeconomic status do share some of the variance explained in participant culture, by far the greatest amount of variance is attributable to their *independent* effects.

Table 8 clearly points to the conclusion that ethnicity has the greater relative impact on participant culture. For the overall participant culture index, ethnicity exhibits more than twice the explanatory capacity of socioeconomic status; for cynicism and community political knowledge, approximately three times; and for civic awareness, ethnicity exhibits an amazing ten times the explanatory power of socioeconomic status. Only for involvement and efficacy are the impacts of the two factors roughly equal (i.e., for involvement, ethnicity explains 9.8 percent of the variance uniquely, while socioeconomic status explains about 6.5 percent; for efficacy, 6.1 percent and 6.3 percent, respectively).

There are two remaining alternative hypotheses that might explain the high predictive capability of ethnicity. First, several of the ethnic groups examined (Cubans and Dominicans, for example) are largely *unassimilated.* Although it was earlier demonstrated that length of residence in New York City has little impact on communal participation or participant culture, other dimensions of assimilation could be affecting our findings. An argument could be made that as intermarriage rates increase, as friendship circles become multiethnic, as attachments to cultural institutions (e.g., religious institutions) break down, and as hispanic groups become more proficient in the use of English, levels of participant culture should be similar for the various ethnic groups in the sample.

A second alternative hypothesis centers on the role of *ethnic political consciousness.* Many students of ethnic politics maintain that high levels of ethnic political involvement are largely a function of two interrelated aspects of consciousness raising: (1) a belief that members of the mainstream society discriminate against one's ethnic group, and (2) a positive identification with ethnic group goals and interests (see, for example, Antunes and Gaitz, 1975; Glazer and Moynihan, 1975, pp. 1–26; Leggett, 1968; Wolfinger, 1965; Verba and Nie, 1972). It might be expected, then, that as ethnic political consciousness increases, a political culture more supportive of participation will develop. Thus,

Table 8. Commonality Analysis: Variances in Participant Culture Explained by Ethnic Identity and Socioeconomic Status (N=379)

Participant Culture	R^2 Unique to Ethnicity	R^2 Unique to Socioeconomic Status	R^2 Common to Both	Total R^2
Overall Index	.218	.094	.143	.455
Awareness	.232	.023	.080	.335
Involvement	.098	.065	.106	.269
Knowledge	.090	.031	.041	.162
Cynicism	.089	.027	.042	.158
Efficacy	.061	.063	.037	.161

Source: Ethnic Block Survey, New York City Neighborhood Project, Bureau of Applied Social Research, Columbia University, 1973.

once ethnic political consciousness is statistically controlled, political culture differences between nominally defined ethnic groups could be small or nonexistent.

I employed stepwise regression analysis to test the alternative theses that variations in levels of assimilation and/or ethnic political consciousness account for differences in participant political culture between ethnic groups. Variables measuring assimilation (i.e., intermarriage rates, inter-ethnic friendship patterns, and religiosity) and ethnic political consciousness (i.e., preferences for ethnic friends and neighbors, beliefs about the need to take political action with ethnic cohorts, and the belief that New York City government officials discriminate against the respondent's ethnic group) were all entered in step one of the regression equation along with socioeconomic status.[12] Nominal ethnic identity was entered in step two. If lack of assimilation or low levels of ethnic consciousness account for participant culture differences between ethnic groups, we can assume that nominal ethnic identity will explain little or no additional variance in participant culture once these variables are controlled, i.e., its additional R^2 will approximate zero.

The assimilationist and political consciousness theses were not confirmed by stepwise regression analysis: ethnic identity continued to explain about 18 percent of the variance in participant culture when assimilation, consciousness, and socioeconomic status were controlled, compared to 22 percent when only socioeconomic status was controlled. In addition, the effects of English proficiency (as measured by language of interview) were examined among the three hispanic groups only ($N = 189$). Although the zero-order correlation (r) between participant culture and English proficiency was .316, when social background variables (nominal ethnic identity, socioeconomic status, sex, age, and length of residence), variables measuring assimilation, and political consciousness variables were controlled, the resultant beta for English proficiency was only .089. Knowing an individual's level of English proficiency, then, adds little to our understanding of variations in participant political culture that is not already explained by other variables in the analysis.

Thus, the thesis that ethnic cultural differences persist and are reflected in political attitudes that predispose individuals to participate in politics continues to receive strong support from the data.

Conclusion: Ethnic Political Culture, Participation, and Inequality

The class-caste controversy has a long and venerable tradition in sociology. At issue in this controversy is the relative influence of social class versus ethnicity as sources of socio-political behavior. For many students of political participation the class-caste controversy appears to have been resolved in favor of social class, or at least in favor of the joint (ethclass) effects of ethnicity and social class. Apparently researchers assume that the homogenizing processes of assimilation and ethnic socioeconomic mobility reduce the effects of ethnic culture to a marginal role in explaining participation behavior.

The findings of this study raise serious doubts about the idea that ethnic cultural influences on participation are withering away. Large differences in levels of participant culture were found to exist between ethnic groups. Controlling for socioeconomic status, age, sex, length of residence, assimilation rates, and levels of ethnic consciousness did not eliminate the cultural effects discovered in correlation analysis. Our analysis also demonstrated that both ethnicity and socioeconomic status make

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[12] Along with socioeconomic status, three other indexes were constructed for control purposes. The "ethnic social cohesion index" is composed of four items: (1) *intramarriage,* i.e., whether respondent is married to a member of the same nominal ethnic group, (2) *ethnic friendship patterns,* i.e., the percentage of one's best friends who share one's ethnic identity, (3) *primary ethnic social intimacy,* i.e., the degree to which one discusses problems *only* with relatives and close friends, and (4) *religiosity,* i.e., the rate of attending religious services at the religious organization modal to the respondent's ethnic group. The second index is labeled "ethnic social consciousness," and is composed of responses to two statements: (1) "I prefer to make friends mostly with other (respondent's ethnic group)," and (2) "I prefer to live in neighborhoods where there are many other (respondent's ethnic group)." The third index is labeled "ethnic political consciousness," and is composed of responses to three items: (1) "(members of respondent's ethnic group) should take political action to help other (members of respondent's ethnic group)," (2) "Sometimes it is necessary for (members of respondent's ethnic group) to use militant tactics to fight for the things (members of respondent's ethnic group) need," and (3) whether or not the respondent, in an open-ended statement, identified his or her own ethnic group as one receiving worse treatment from New York City government than other ethnic groups. Variables were standardized and combined to form three separate indexes.

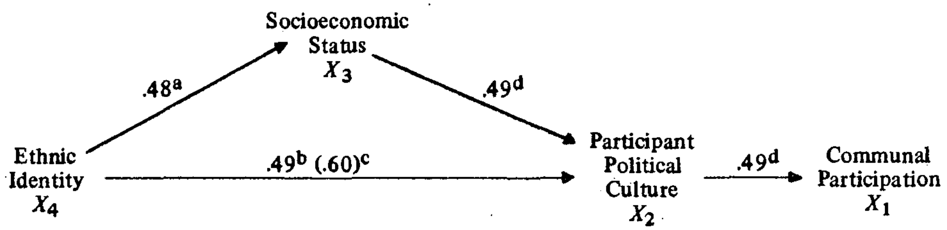
independent and powerful contributions to the explanation of participant culture. Ethnicity, however, proved to be a *better* predictor of participant attitudes than socioeconomic status.

The data reported in Figure 1 provide strong evidence for the importance of ethnic political culture. Socioeconomic status and nominal ethnic identity are shown to have minimal direct effects on communal participation once participant political culture is controlled. However, both ethnicity and socioeconomic status have powerful effects on participant culture, as demonstrated by the size of the path coefficients in the diagram. Further, after controlling for ethnic identity, the resultant partial beta for the relationship between socioeconomic status and participant political culture was .35. Since the corresponding partial beta for the relationship between ethnic identity and participant culture (i.e., controlling for socioeconomic status) was .49, we can conclude that ethnic identity has a greater direct effect on participant culture than socioeconomic status.

The findings reported in Figure 1 also lead to a reconsideration of the adequacy of Verba's and Nie's "Standard SES Model" of participation. It is no doubt accurate as far as it goes, but other factors ignored by the standard model (like ethnicity) may add a great deal to our understanding of participation and political inequality. The Verba and Nie standard model clearly symbolizes the consensus that has developed on the primacy of social class as the fundamental source of variation in levels of political participation. By placing their findings in the context of social inequality in America, they make a convincing argument that upper-status individuals have disproportionate access to political decision makers, and that such access can lead to inequality in the distribution of benefits.

While Verba and Nie do examine other potential sources of political inequality (like sex, age, party affiliation, and organizational membership), their analysis fails to give serious attention to inequality that is not directly linked to social class. True, they do analyze the role of race in political participation, and race is certainly an important dimension of ethnicity, but the authors' treatment of race is restricted to its relationship to socioeconomic status and race consciousness. Obviously, whites and blacks are differentiated along class lines, which explains many differences between the races in political participation. Race consciousness can also serve as a resource for increasing rates of black participation. But race is only one dimension of ethnicity, and socioeconomic status and social discrimination are only two factors leading to political inequality between ethnic groups.

This study, with its focus on ethnic political


Socioeconomic Status X_3
$.48^a$
$.49^d$
Ethnic Identity X_4
$.49^b$ $(.60)^c$
Participant Political Culture X_2
$.49^d$
Communal Participation X_1

Prediction Equation	Actual Values
β 13.2=0	0.11
β 13.24=0	0.11
β 14.23=0	0.10^b

Source: Ethnic Block Survey, New York City Neighborhood Project, Bureau of Applied Social Research, Columbia University, 1973.

[a] The one-way arrow leading from ethnic identity to socioeconomic status reflects the assumption that one's ethnic identity affects one's life chances, including economic opportunities. It is in this sense that ethnic identity can be viewed as a "cause" (albeit not the only one) of socioeconomic status. Clearly, socioeconomic status cannot be viewed in any sense as a cause of nominal ethnic identity.

[b] Partial beta obtained from multiple classification analysis (MCA).

[c] Coefficient in parentheses is a Pearson R.

[d] Coefficient is a Pearson r (i.e., $p12=r12$; $p23=r23$).

Figure 1. Path Model of Communal Participation

culture, goes beyond Verba and Nie in addressing the relationship between participation and social inequality. Since people are predisposed to participate in politics because of certain political attitudes, these attitudes can be viewed as *political resources* that are unequally distributed. Political cultures strongly supportive of participation can also be viewed as *group* resources. Ethnic groups whose members exhibit such strong participant cultures stand to benefit politically because their members are more politically active. On the other hand, ethnic groups with weaker participant orientations may well be disadvantaged vis-à-vis benefits distributed through the political system. Discrimination and low socioeconomic status have been demonstrated by others to limit the opportunities of some ethnic groups to receive benefits from the political system. Weak participant political cultures may well be a third basis for political and therefore social inequality.

Several authors have identified ethnicity as an important source of political participation. Among the best-known works on the subject are those of Greeley (1974) and Wilson and Banfield (1965, 1971). Using Verba's and Nie's data, Greeley maintains that ethnicity is "a meaningful predictor of political participation in American society" and that "its impact does not go away when social class is held constant" (p. 170). He pessimistically concludes that "given the poverty of research done on American ethnic groups, it is impossible to generate any hypotheses about their political participation" (p. 173). While my findings support Greeley's contention that ethnicity is a meaningful predictor of participation, even when socioeconomic status is held constant, I do take issue with Greeley concerning the possibility of explaining ethnic differences in participation. My theory, admittedly a partial one, proposes a political culture explanation that links ethnicity to communal participation.

The works of Wilson and Banfield are particularly relevant here, because they also argue a cultural thesis in relation to ethnic participation. However, their "political ethos" theory is significantly different from the ethnic culture argument of this article. The authors present the rather questionable thesis that working-class white ethnic culture, nurtured historically by the urban political machine, is a "private-regarding" political ethos (i.e., it stems from a private, personalistic motivation rather than a "public regarding" or community-minded motivation). The political culture thesis argued here deals not with private versus public *motivations* for political behavior; rather, it contends that ethnic groups vary in participant culture supportive of participation. Furthermore, Wilson and Banfield posit an explicitly "ethclass" argument that links their "private-regarding" ethos to working-class populations. They therefore fail to consider the separate impacts made by socioeconomic status and ethnicity on political participation.

The discovery of cultural differences between ethnic groups does not necessarily lead to a pessimistic conclusion about the future of ethnic political equality in America. Cultures do change. Low or high levels of participant culture are not frozen into a group's existence. But cultural change is slow if such change is the result only of individual adaptation. Dramatic events, increased organizational activity, and mobilization can be especially important for enhancing levels of participant culture. Black Americans, for example, may have acted as a prototype for other disadvantaged groups, and there is evidence that the experience of blacks has had a kind of "demonstration effect" on American Indians, Chicanos, and Puerto Ricans during the 1960s and 1970s. In addition, high levels of ethnic political consciousness and increased social interaction with members of the broader community can both lead to increased political involvement. Thus there are a number of factors that can affect participant political culture, and such changes need not come about slowly.

However, if ethnic divisions are going to be enduring features of the socio-political landscape, social scientists and policy makers need to understand more fully the nature and implications of such divisions. To date, public policy has responded to ethnic inequality primarily in terms of improving opportunities for better education, higher income, and more satisfying jobs, while concurrently seeking to ameliorate social discrimination through legal redress of grievances. In the future, it may be necessary for decision makers to address inequality in terms of political culture resources, particularly those that inhibit political participation.

At the level of theory building there is a strong need to clarify the role played by ethnicity in political participation. In particular, theorists should focus more closely on the identification of factors which create and maintain socio-political inequality among ethnic groups. It is already clear that there are social and economic sources of ethnic political inequality. It may become increasingly clear that a low level of participant culture is yet another source of political inequality among ethnic groups, and one which is in need of serious attention if inequality based on ethnicity is to be reduced or eliminated.

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The Development of Party Identification among Adults: Exploration of a Functional Model

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This article proposes a model for the systematic development of adults' party identification, based on voters' need for a way to handle difficult electoral decisions. Several variables are noted which should heighten this need, thus making it more likely that voters will develop party identification. The model is partially tested, in an exploratory way, by analysis of panel data from the United States and Britain, and by cohort analysis of United States elections from 1952 to 1976. I develop the following implications of the model: (1) the "life-cycle" process by which party identification increases with age may be largely a function of difficulty in meeting information costs; (2) the process by which party identification, once it exists, becomes stronger appears to differ from the process by which voters move from independence to identification; (3) class-consciousness, in the presence of class parties, may obviate the need for direct identification with parties; (4) the American electorate appears increasingly to be one in which political change may occur regularly, rather than through the fitful process of realignment.

Party identification has been much studied since the key work of *The Voter Decides* and *The American Voter* (Campbell et al., 1954, 1960) but its causes have not been much studied. Researchers have described the nature of party identification, its development and its effects, but the fact that most Americans are attached to a party has seldom been taken as an anomaly to be explained. On the contrary, this has generally been treated as a natural situation, though when party identification began to decline in the late 1960s, the *decline* was treated as an anomaly. This acceptance of party identification as a natural thing is puzzling, since it flies in the face of the individualistic liberalism on which most American assumptions about politics are based.[1]

Perhaps one reason why party identification has been treated as a natural condition is that its sources have been thought to reside largely in childhood, a time when people are not yet much involved in politics. This has precluded most political explanations, and has suggested instead parental transmission as the major process by which people become partisans. Research has concentrated on rates of intergenerational transmission, on the age at which party identification first appears, on which parent is the chief socializing agent, and so on. (For an excellent review of this literature, see Sears, 1975.) This body of research has done a great deal to clarify the circumstances surrounding children's acquisition of party identification, but it has not allowed us to address the basic question: Why do people identify with parties?

Though research has concentrated on children's acquisition of party identification, it is not necessarily true that the roots of adult party identification lie in children's party identification. Evidence on this question—pro or con—is simply lacking. We know that adults frequently identify with the party their parents identified with, but it is not clear whether this is because of direct transmission, or because the children share their parents' social situation and many of their basic values; either of these might eventually lead them to choose the party their parents had earlier chosen. Goldberg (1969) provides some evidence for the latter of the two processes.

Similarly, we know that children report party identification, and that their reports are fairly stable; and we know that adults report party identification, and that their reports are fairly stable. But no one has as yet been able satisfactorily to link childhood party identification with adult party identification—admittedly a massive problem of design and analysis. The best evidence on this question is that of Jennings and Niemi (1978), who followed a panel of adolescents aged 17–18 in 1965

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[1] Two exceptions to this general rule are Beck (1975) and Goldberg (1969), who treat political realignments and social position, respectively, as sources of party identification.

through to 1973, when they were 25 and 26. Along with this panel, they followed a panel of the respondents' parents. Thus, they were able to ascertain how thoroughly the party identification of people in what might be called their "late childhood" carried over into the early years of their adulthood. They found (1978, p. 349) that the relationship between adolescent party identification and young adult party identification, over the eight-year span, was $T_b = .42$. By way of comparison, the stability of the parents' party identification over the same time span was $T_b = .67$. Though some of the adolescents may have in fact been "adult" at 17 or 18 years, T_b of .42 is large enough to show a relationship between childhood party identification and adult party identification. However, it is also small enough, compared with T_b of .67 for the sample of adults, to suggest that much of the formation of adult party identification may occur in the adult years. Thus there is simply not a clear verdict on how much of adult party identification can be traced to childhood.

Further, recent research has found more systematic changes in party identification among adults than had previously been expected (Brody, 1977, 1978). This should encourage us to look to adult situations for explanations of adult party identification.

Adult Partisan Development

The most generally recognized model of adults' development of party identification is the so-called "life cycle" model which was first put forward in *The American Voter* (Campbell et al., 1960, pp. 161–65), and buttressed in Converse's elegant "Of Time and Partisan Stability" (1969). This model holds simply that through their adult years, people become less independent as they age and, if they already identify with a party, identify with increasing strength. This is generally explained, as in *The American Voter*, as a reinforcement process. There has been a running debate over whether the model actually describes what happens across the life cycle (Glenn, 1972; Glenn and Hefner, 1972; Abramson, 1976, 1979; Converse, 1976; Shively, 1979), but what is important for our purposes is that the model largely does not address the question which was set out in the opening paragraph, "Why should people identify with a party?" Though much of the original relationship in the American data is due to movements away from independence (Campbell et al., 1960, p. 162), and much of the later literature—especially that which is based on Gallup data—has addressed solely movements between partisanship and independence, the model does not offer an explanation for such movement. Reinforcement may explain why people who already identify with a party should learn to identify with it more strongly, but it cannot explain why independents should choose to identify with a party in the first place (Claggett, 1979). How can what does not yet exist be reinforced?

How then may we address the question of why people identify with parties? A functional model of party identification, based on individuals' needs which are satisfied by party identification, would seem an appropriate vehicle. That party identification may fill needs for individuals has long been recognized. Among those who have recently studied voting, Downs (1975), Campbell et al. (1960), and Goldberg (1969) have particularly emphasized this. While Goldberg is concerned with the virtues of choosing one party rather than another, both Downs and the authors of *The American Voter* described a function which might account for the presence or absence of party identification, per se. This is the party's function of providing political cues to voters who feel themselves in need of guidance because they must make political decisions under confusing circumstances. (In the interest of brevity, I shall refer to this as the "decisional function.")

Though the existence of the decisional function is noted by these and later authors, it has been treated as an effect of party identification, rather than as a need which might draw people into party identification. (Discussion of the function in *The American Voter*, for instance, occurs in a chapter entitled "The Impact of Party Identification.") Treated as a need, the function may provide an explanation for adults' acquisition of party identification. Treating it in this way also leads us to specify how the function operates under varying circumstances; the function may thus provide the basis for a somewhat broadened model of the development of party identification among adults.

A Functional Model

Five variables seem likely to affect the production of party identification through the decisional function: (1) the strength of the feeling that one must participate in politics (I shall deal only with voting, but the same analysis could apply also to other forms of political participation); (2) the cost of information pertinent to the voting choice; (3) resources available to the voter to pay those costs; (4) concern for the quality of the decision; (5) the availability of other, more efficient, decisional shortcuts. Let me expand on these:

(1) In order for the process to operate at all, individuals must feel some need to make voting decisions. Presumably in the eighteenth century most people would not have felt such a need. The spread of the franchise and of the expectation that it should generally be exercised must have spread this need through most of the populations with which we are concerned in studies of voting behavior. However, it is not necessarily true that this need is felt equally strongly by everyone (it obviously is not, since some people do not vote), or that it is equally widespread in all countries, or that it is equally widespread in any given country throughout that country's history. *The more strongly the need to participate is felt, ceteris paribus, the more the decisional function will tend to produce some guide, such as party identification.*

(2,3) To the extent that information pertinent to the choice which must be made is "expensive," voters will seek out efficient shortcuts to a decision, such as party identification. (While none of the variables presented here is entirely original to this paper, the notion that information costs will lead people to identify with parties comes quite directly from Downs, 1957.) "Expense" may be literal (the cost of a newspaper or television set) or non-monetary (the strain of dealing with conflicting bits of information, the difficulty of organizing and remembering political facts, and so on). Again, these costs are not constant either for individuals or for nations. The explosion of "news coverage" in the last few decades has surely made information cheaper. And at least one individual characteristic, the level of formal education, must increase the resources available to some people for gathering and organizing information, as compared with others. (2) *Ceteris paribus, the more "expensive" political information is, the more the decisional function will tend to produce a guide such as party identification; and (3) the lower the resources available to individuals to pay those costs, the more the decisional function will tend to produce such a guide.*

(4) The level of quality desired in the decisions must affect the individual's choice between party identification and some other way of deciding how to vote. To the extent that a person does not care a great deal whether the vote is cast wisely or not, low-information shortcuts such as party identification will be relatively attractive. At *very* low levels of concern, shortcuts with even less informational content than party identification might be chosen; for instance, the voter might simply flip a coin. The degree of concern for the quality of decisions must vary to some extent among individuals and across space and time. Of available guides, party identification should generally involve a relatively low level of information; a "responsible party system" would presumably provide the major exception to this. *Ceteris paribus, the tendency of the decisional function to produce party identification will be related to the individual's concern for the quality of his or her decision. A moderately low level of concern—but not the very lowest—will be associated with the development of party identification.*

(5) Finally, let us consider the fifth variable affecting the production of partisanship by the decisional function. Conditions (1) through (4) have been concerned with whether individuals will feel the need to equip themselves to make voting decisions, and with whether they will choose to meet that need by becoming informed or by seeking a shortcut through the mass of political information. I have referred only to one kind of shortcut, party identification, but the arguments I have advanced so far could apply equally well to other shortcuts.

There are many modes of decision which could provide shortcuts. One which voters have often used is to pick some significant person—a spouse, for example, or union head, or local political boss—as a leader and vote as that person suggests. Another shortcut, which is possible if class-consciousness is high, is to vote according to the perceived interests of the class. In this case, class rather than the party which represents that class, serves as the organizing shortcut. (It seems likely, for instance, that the weak direct ties to political parties in postwar Europe have resulted from the prevalence of strong ties to a class or religion, which have obviated the need for direct ties to any party, per se.) *Ceteris paribus, if there is available to the voter some other shortcut to decisions which is closer to his or her preferred combination of information costs and decision-quality than identification with any of the parties, then the voter will not identify with a party. Instead, the voter will use the alternative shortcut.*

When a voter uses a shortcut other than party identification, he or she may still be associated with a party. While some shortcuts, such as selling the vote each time to the highest bidder, do not provide the individual with a lasting association with any particular party, most shortcuts do. Voting according to the interests of one's class, for instance, requires one to vote for the party which represents the class. For organizational reasons there is likely to be considerable continuity in the relationship between political parties and classes; therefore, class voting will result in a prolonged association of voters with certain political parties.

Such association differs from party identification, however, in that there is no direct tie to the party. The party itself is not a guide to voting choice, but is rather an expression of that choice. Expressed partisanship, then, will be synonymous with the vote and parties as such will not serve as guides to organize behavior. Under the proper organizational circumstances, support for such parties may be quite volatile, if various parties compete to represent the same class (Shively, 1972, pp. 1220–25).

The five statements above comprise a model of the following sort: Statements No. 1 through No. 4 identify four conditions which, in interaction with each other, will lead individuals to seek out some sort of decisional guide. The four conditions operate in *interaction* with each other because, to the extent that one of them is weak or absent, the others will not operate as strongly to make people seek a decisional guide. For those quite unmotivated to vote, for instance, the magnitude of information costs associated with voting would not matter much. Or, if "perfect information" were available, no matter how motivated a person were to vote, there would be no need for him or her to develop an identification with a party.

The first three conditions (motivation, magnitude of information costs, and capacity to bear information costs) will be monotonically related to the propensity to identify with parties, all other things equal. The fourth condition, concern for quality of the decision, will bear a U-shaped relationship to the propensity to identify with parties. If one has little or no concern for the quality of the vote, there will be no need to develop any decisional guide. At a higher, but still low level of concern, one would presumably be likely to develop party identification as a decisional guide. At a still higher level of concern, one might be expected to develop some other sort of guide which yielded decisions of higher quality, or perhaps to eschew guides altogether.

The fifth condition (the availability of alternative decisional guides) stands rather apart from the first four, since it is not related to the need for some sort of decisional guide. Rather, if some guide is needed, it will affect the probability that party identification is the guide which develops.

A Test of the Model

I shall present here only a partial test of the model, a test of statements 1 and 3: "The more the need to participate is felt by a voter, the more strongly the decisional function will operate to produce some guide such as party identification." "The lower the resources available to the voter to pay information costs in voting, the more strongly the decisional function will operate to produce a guide such as party identification." The major test will consist of analysis on the 1956–60 and 1972–76 Center for Political Studies panels; a complementary test will follow a cohort from 1952 to 1976, analyzing net changes in partisanship by education.[2] A test of statement 5 of the model appears below (pp. 1049–50).

For the 1956–60 and 1972–76 panels, I have calculated the conditional probability, for each category of respondent examined, that a nonpartisan in 1956 (or 1972) would have identified with a party in 1960 (or 1976). Similarly, I have calculated the probability that one who identified with a party in 1956 (or 1972) would have shifted to independence in 1960 (or 1976). The model predicts that for individuals with a low capacity to handle information costs and for highly motivated individuals—and especially for individuals who have both of these characteristics simultaneously—the probability of a shift into party identification should be high relative to a shift into independence. It is difficult to devise a summary measure for these two transition probabilities, but one measure is suggested by the fact that such a pair of probabilities is associated with a unique level of party identification which, if the probabilities remained constant over a series of transitions, would be reached and would be maintained at equilibrium (Coleman, 1964, pp. 103–12). For instance, from 1956 to 1960, 62 percent of independents who had had less than eight years of education shifted into party identification, while only 11 percent of party identifiers at that level of education shifted into independence. These two transition probabilities tend towards an equilibrium level of .85 as the proportion identifying with a party. The single number, .85, may thus summarize the joint tendency of the two transition probabilities. It is this number, the equilibrium point for the two transition probabilities, that I shall use as the dependent variable in the tests with panel data below.

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[2] The data used here were made available by the Inter-University Consortium for Political and Social Research. The data were originally collected by the Survey Research Center Political Behavior Program, now the Center for Political Studies at the University of Michigan. Neither the original collectors of the data nor the Consortium bear any responsibility for the analyses or interpretations presented here. CPS uses the following questions to indicate if a respondent has acquired a party identification: "Generally speaking, do you usually think of yourself as a Republican, a Democrat, an Independent, or what?" If a party identification is indicated, a followup question asks: "Would you call yourself a strong ______ or a not very strong ______?" Independents are also asked if they "lean" to a particular party. For the present analysis I have defined "partisans" as all those who responded with a party to the first question. There is, of course, controversy about this, but I prefer to interpret the question as literally as possible, since this seems to me to be the most likely way that respondents will have interpreted it. For an argument that leaners may often simply be stating their intended vote when asked how they "lean," and thus should be treated as genuine (if decisive) independents, see Shively, 1977, pp. 16–20.

Note, incidentally, that the two probabilities given in this example represent a very considerable systematic movement of adults between independence and party identification, considering that the shift occurred between just two elections. More than half of the independents moved to identify with parties, with only a small movement in the other direction. As we shall see below, systematic movement of this magnitude appears to be the rule for those groups of the population identified by the decisional function model as most in need of a decisional guide.

The two tests of the model are presented in Figure 1. Very rough measures of information costs and motivation are used to predict to the equilibrium point of party identification. Education is used as an index of individuals' capacity to bear information costs, and the "citizen duty scale" is used as an index of motivation to vote. (For a description of this scale see Rusk et al., 1968, Vol. 1, pp. 461–62). The prediction from the model is that equilibrium points should be positively related to the citizen duty scale and negatively related to education, with an interaction effect (higher expected equilibrium points) where low education coincides with higher scores on the citizen duty scale.

The results of the test are displayed in Figure 1. The prediction of the model with regard to education is borne out cleanly, as is the prediction of an interaction between education and motivation.[3] The weights of transition

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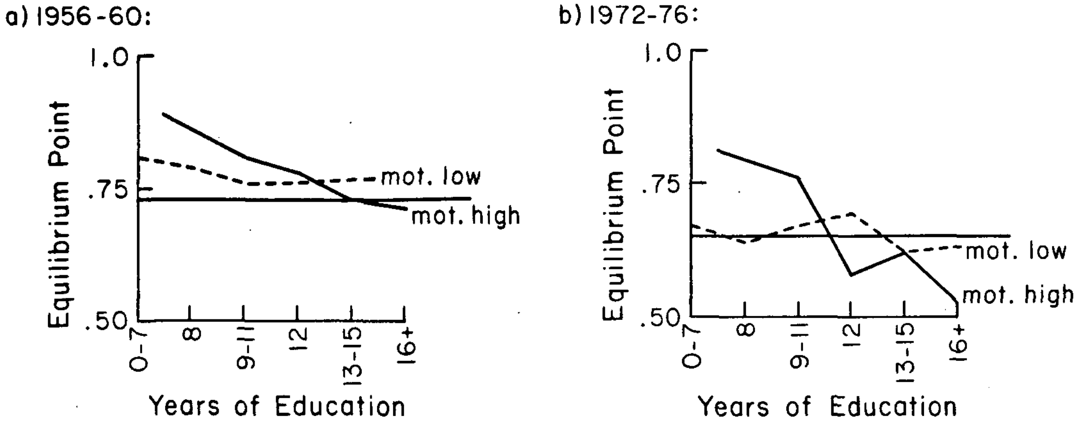
[3] It might be objected that the effect is accomplished only by the small numbers of cases at the lower end of education. However, the electorate still includes substantial numbers of people with little education. Of the 1956–60 panel, 27 percent had had eight years of education or fewer; the comparable figure for the 1972–76 panel is 17 percent.



Source: 1956–60 and 1972–76 panel studies of the national electorate conducted by the Center for Political Studies, University of Michigan.

Note: Motivation is "high" if the score on the citizen duty scale is 4; for any lower score, motivation is "low." Due to small numbers of cases, 0–7 and 8 are merged for high motivation in both panels, and 13–15 and 16+ are merged for low motivation in the 1956–60 panel. For N's and conditional probabilities, see Appendix A.

Figure 1. Equilibrium Points for Moves between Identification and Independence, by Education and Motivation

into party identification relative to transition out of party identification, as summarized by the equilibrium point to which they tend, decline steadily with education among the highly motivated in both panels. Among the less motivated there is little if any relationship to education.

The prediction of the model with regard to motivation is *not* borne out, except through the interaction with education. At low levels of education, the more highly motivated tend towards markedly higher equilibrium points, but at high levels of education, the relationship is reversed. Overall, there does not appear to be any direct effect of motivation on the tendency to party identification.

For each panel, I have inserted a horizontal line indicating the actual proportion of the panel identifying with a party at the first of the two elections (.73 in 1956, .65 in 1972). For the 1956–60 panel, all but two categories of voters tended towards equilibria greater than the current level for the panel as a whole. This would be consistent with a general increase in party identification with age, as described in the "life-cycle model." Note, however, that the extent of this "life-cycle" effect appears to be in part dependent on the terms of the decisional model; I will return to this point below. For the 1972–76 panel, though the general form of the relationships is about the same, everything has dropped lower; presumably this is a result of negative period effects. Now several categories fall below the current level of party identification.

While the differences shown in the figure do not look dramatic, this is partly due to the conservative nature of the summarizing measure. For instance, for highly motivated voters in the 1956–60 panel, the equilibrium point for those with eight years or less of education is .89, while the equilibrium point for college graduates is .71. This difference does not appear to be large, but it summarizes the fact that over the four years of the panel, of those with eight years of education, 62 percent of independents moved to party identification, while only 8 percent of party identifiers moved to independence—a difference of 54 percentage points. At the same time, of the college graduates, 29 percent of independents moved to party identification, while 12 percent of identifiers moved to independence—a difference of 17 percentage points. Thus in this case equilibrium points of .89 and .71 correspond go percentage differences of 54 and 17. Particularly considering that we are looking at a period of only four years, the contrast in the movements of these groups of voters is striking. A full list of proportional movements is given in an appendix.

The fact that the test produces results of a similar form in the two panels adds to our confidence that the results are not accidental. More importantly, it suggests that the process set out in the functional model continues to operate in the 1970s much as it did in the 1950s, even though in the later period something has clearly been working generally to discourage moves into partisanship.

A complementary test adds further evidence of this. In Figure 2 the percent partisan for different educational levels of a large cohort is traced as that cohort aged from 1952 to 1976. The cohort consists of all white respondents aged 21–48 in 1952, aged 25–52 in 1956, and so on up to those aged 45–72 in 1976. I have included only whites because the special historical experience of blacks across this period might have led to changes in party identification which could have been confounded with those caused by the decisional function.

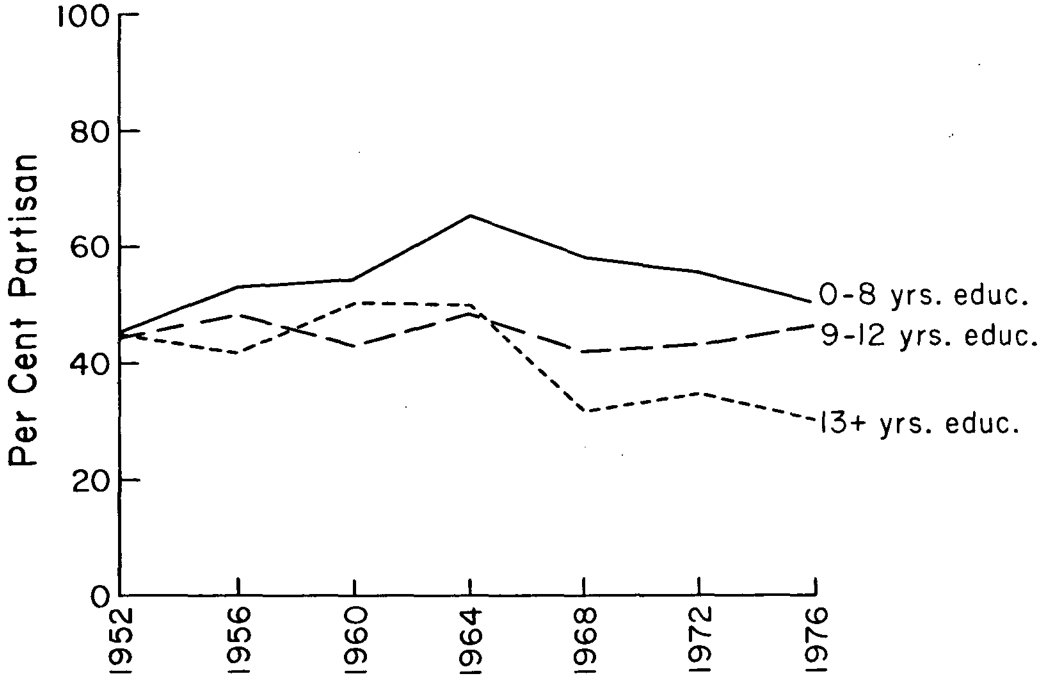
Though any individual point on the graph in Figure 2 is of only moderate reliability (N's on which the percentage are based range from 162 to 600), there is a pattern of continuous differentiation by education, in the expected direction. In 1952, the three educational groups did not differ. As time passed and the respondents aged, the educational groups pulled apart fairly steadily, with the least educated groups becoming more partisan relative to the others.

The changed circumstances for party identification in the 1970s are evident in the graph. From 1952 to 1964 the groups generally increased in partisanship, while from 1964 to 1976 they generally decreased. But differentiation by education, which is presumably owing to the decisional function, continued during *both* the periods of increase and decrease.

This cohort analysis is only partially independent of the panel analyses; some of the 1956 and 1960 respondents were members of the 1956–1960 panel, and some of the 1972 and 1976 respondents were part of the 1972–1976 panel. However, it is a largely independent test, which suggests again that the decisional function is robust across quite different historical circumstances.

Implications of the Model

If this partial test of the model at least establishes it as a plausible candidate for an explanation of why adults come to identify with parties, it is worth considering what



Source: 1952, 1956, 1960, 1964, 1968, 1972 and 1976 presidential election studies conducted by the Center for Political Studies, University of Michigan.

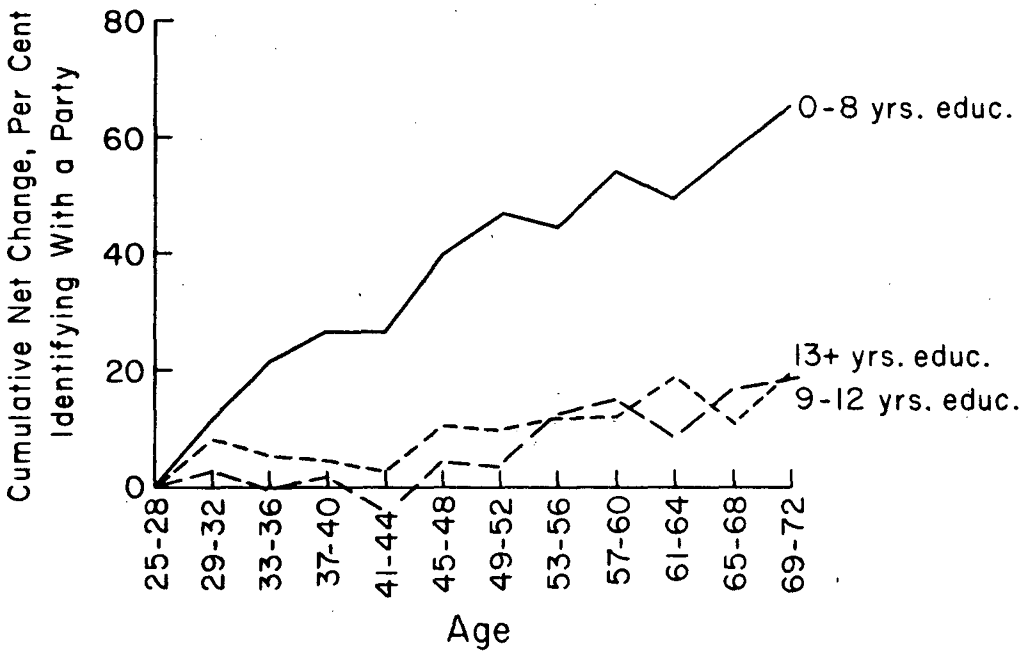
Figure 2. Development of Partisanship, 1952–1976, among a Cohort of Whites Aged 21–48 in 1952

insights the model can provide into electoral politics. Let me present here some suggestions about the "life-cycle model," about the strengthening of party identification, about class-voting as an alternative to party identification, and about historical variation in electorates and the potential for realigning elections.

The Life-Cycle Model. The idea that as adults age they are increasingly likely to identify with parties, and to identify with them strongly, has been important in theories of voting behavior. Both Converse (1969) and Przeworski (1975), for example, have incorporated such a process as an important assumption in models of electoral development. While the strengthening of party identification when it is already present may be in part a function of selective reinforcement (a point to which I shall return), as I have argued, no explanation has been offered as to why adults should progressively identify with parties in greater *numbers* as they age. Our analysis of the decisional function suggests a set of conditions which could cause movements of this sort, and it further suggests that such movements should not be uniform but should come differentially from among the most vulnerable portions of the population. This is clearly implied in Figure 2, but we can look at the process more systematically.

Cohort analysis of the electorate from 1952 to 1976 may allow us to examine the process, at least with regard to the prediction that life-cycle gains in the proportion of the population identifying with parties should come disproportionately from among the least educated.

Figure 3 presents results of a cohort analysis of the American electorate from 1952 to 1976, separated according to their education. This analysis proceeded as follows: each education level was divided at each election into four-year cohorts by age (25–28, 29–32, etc.). Change in the percent identifying with parties was measured for each cohort over each pair of elections from 1952 to 1976. (Thus, I measured change from age 25–28 to 29–32 from 1952 to 1956 by subtracting the percent of 25 to 28-year-old voters identifying with parties in 1952 from the percent of 29 to 32-year-old voters identifying with parties in 1956.) For each education level at each age, then, there are



Source: 1952, 1956, 1960, 1964, 1968, 1972 and 1976 presidential election studies conducted by the Center for Political Studies, University of Michigan.

Note: The life cycle starts at age 25 rather than at age 21, because of the problem in sampling voters in the 21–24 age group noted by Converse (1976, pp. 47–50). Blacks are excluded because of problems noted by Abramson (1979).

Figure 3. Development of Party Identification across the Life Cycle, by Education

six estimates of their change over four years of aging, one estimate for each contiguous pair of elections from 1952 to 1976.

One cannot simply average these six estimates to calculate the effect of aging, however, because in addition to whatever effects were associated with their aging, voters over the latter half of the period were also pulled in the direction of independence by historical forces. Actual change would be a combination of these historic forces ("period effects," in the terminology of cohort analysis) and of change associated with aging. This is, in other words, the frequently encountered identification problem in cohort analysis (Mason et al., 1973).

In order to separate effects associated with aging from period effects, I developed criterion groups for each education level, in order to measure pure period effects. These groups were constructed so as not to age over a given pair of elections, nor to change significantly through generational replacement. For each pair of elections, I compared the percent identifying with a party at the first election among voters who had entered the electorate in 1944, 1948, 1952 or 1956, with the percent identifying at the second election among voters who had entered the electorate in 1948, 1952, 1956 or 1960. These two groups were of approximately the same age; and, assuming that the 1944–1960 period was one of more or less constant impulse towards partisanship, they should not have differed much generationally among themselves. Thus, any change from the first group to the second group must be due almost solely to period effects occurring over the pair of elections. For the period 1964 to 1968, for those with 0–8 years of education, for example, the estimate of period effects is –.8 percent. For the cohort of 0–8 years education who were 37–40 years old in 1964, total change from 1964 to 1968 was –1.25 percent. Subtracting the estimate of period effects from this leaves an estimate of effects purely associated with aging of –.45 percent. I corrected each cohort change for estimated period effects in this way, and then averaged the six estimates for each age/education cohort. In the averaging, I weighted estimates by the number of cases on which they were based.

(For a fuller presentation of the correction for period effects and for justification of the required assumptions, see Shively, 1979.)

Thus this large base of data was finally distilled down to a single estimate of change associated with four years of aging, for each of eleven age groups at each level of education. The results are presented in Figure 3. Changes at different ages are arrayed cumulatively, so as to imitate a life-cycle. The results indicate that across 44 years of adult aging, the percent of persons with eight or fewer years of education identifying with parties could be expected to increase by about 65 percent. Those with more education might be expected to increase by a bit less than 20 percent. By implication, this suggests that that part of the life-cycle which involves acquisition of party identification (as distinct from its strengthening) is largely caused by the workings of the decisional function.[4]

The Strengthening of Party Identification. Party identification has generally been conceived as a simple dimension involving both the identification itself and the strength of one's feelings about the party with which one identifies. However, identification and its strength are potentially separate phenomena and should be treated separately, since it appears that they may develop by different processes (Van Wingen and Valentine, 1978; Claggett, 1979). I have argued above, for example, that reinforcement might be involved in the strengthening of party identification over time, but that it could not be a factor in the original acquisition of party identification. The model based on the decisional function states only who should be most attracted to party identification; it does not necessarily state who, once identifying, will develop strong partisan feelings. Of course, acquisition and strengthening will be related, as those who had been most in need of partisanship would presumably be relatively more pleased by it, and should tend to develop "strong partisanship" out of that pleasure. But political parties are not neutral vessels for individuals' psychological needs. Parties make policies, and present themselves symbolically, in ways which may or may not be pleasing to their followers. One person might have been led into identifying with a party because of a felt need for guidance, and yet never find that party so pleasing that he or she developed enthusiastic loyalty to it. Another, even one with less initial need for the party, might be greatly pleased by its policies and by its presentation of itself, and become an enthusiastic partisan. In other words, the depth of one's original need for a party guide should affect the pleasure taken in that party and the strengthening of one's identification with it, but many other things will do so as well. The decisional function should lead to the strengthening of party identification, but other factors will also be involved.

It may be interesting to look at the strengthening of party identification among different educational groups in this light. To the extent that a party presents itself as a party of the lower social classes, its class nature should reinforce the operation of the decisional function. The least educated should strengthen in partisanship both because of the class benefits (tangible and symbolic) that they receive from a party, and by virtue of the fact that they find party identification, per se, especially satisfying. An upper-class party, however, should show a mix of two contradictory processes. From its class nature we should expect a positive relationship between education and strengthening, while from the decisional function we should expect a negative relationship. Depending on the relative force of the two processes, the resulting curve might be positive, U-shaped, or (relatively weakly) negative.

Figure 4 presents the relationship between education and the equilibrium points for probabilities of moves between "not very strong" or "fairly strong" partisanship on the one hand, and "very strong" partisanship on the other, based on the 1964–1966 and 1966–1970 British Butler-Stokes panels.[5] Equilibrium points are calculated here as they were above for the probabilities of moves from identification into independence and from independence into identification; only those respondents are examined who identified with the same party at both stages of the panel.

The results are as one would expect, given the strong class biases of the two parties. From

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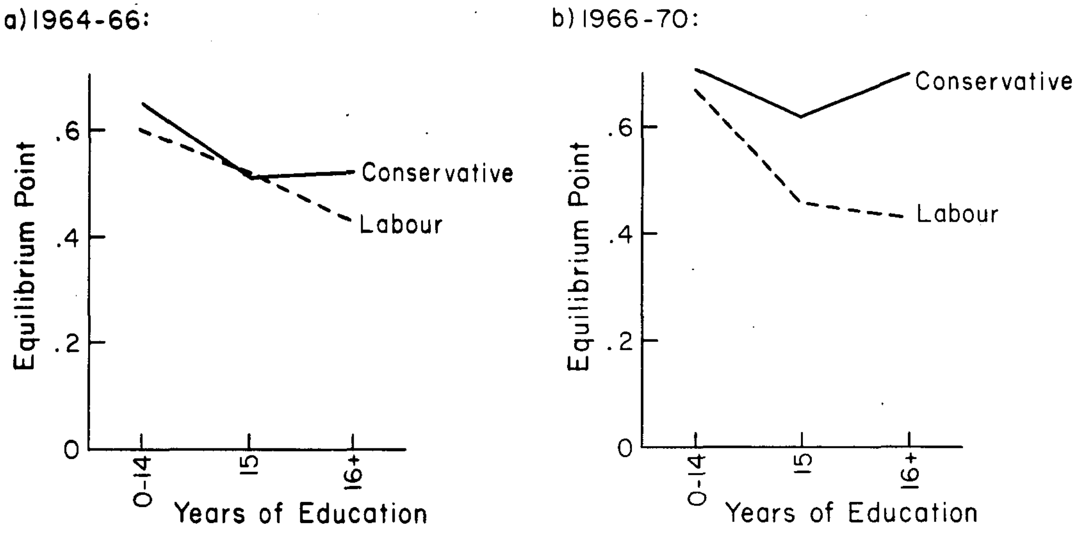
[4] For any who may be uncomfortable with the correction for period effects, a similar analysis done on the "steady-state" period of 1952–1964, with no correction for period effects, gives similar results. The evidence of those elections indicates that persons of 0–8 years of education would show a net gain of 49 percent over 44 adult years. Similar figures for those of 9–12 years and more than 12 years of education, respectively, are 15 and 8 percent.

[5] The data were made available by the Inter-University Consortium for Political and Social Research. They were originally collected by David Butler and Donald Stokes.



Source: 1964–66–70 panel study of the British electorate conducted by David Butler and Donald Stokes.

Note: For N's and conditional probabilities, see Appendix B.

Figure 4. Equilibrium Points for Shifts between Weak and Strong Identification, Britain

1964 to 1966, the curve for the Conservatives (for whom class interest and the decisional function pull in opposite directions) is U-shaped, dipping to the right, while from 1966 to 1970 it is U-shaped and fairly symmetric. For the Labour party class interest and the decisional function pull in the same direction, and the curve for Labour is negative across both panels.

One interesting question regards the class appeal of American parties. It is not clear what we should expect here, and an examination of strengthening in the context of the decisional function may help us assess the overall class appeal of the two parties.

Figure 5 presents analyses of strengthening for American parties parallel to the analysis for British parties presented in Figure 4. Instead of looking at equilibrium points for the probabilities of moving into and out of identification, as in Figure 1, we here examine the equilibrium points for the probabilities of shifting between "strong" and "not very strong" identification. From this figure it appears that *both* American parties are basically upper-class parties.[6] The curves for both parties across both panels are either positive or U-shaped. It is also apparent that the upper-class bias of the Republicans is stronger than that of the Democrats, since across both panels the Republicans tip more to the highly educated than do the Democrats (the difference is slight for the 1956–1960 panel, considerable for the 1972–1976 panel). The dominant impression, however, is of a system of two upper-class parties.

Incidentally, this result may help to explain a minor puzzle in studies of American party identification, the fact that Republicans have been found in almost every study to identify more strongly with their party than Democrats, and to be more loyal to it in their voting.[7] An examination of Figure 4 suggests that this may be owing to the fact that Republican supporters are concentrated in the most highly educated group, the group most likely to strengthen in

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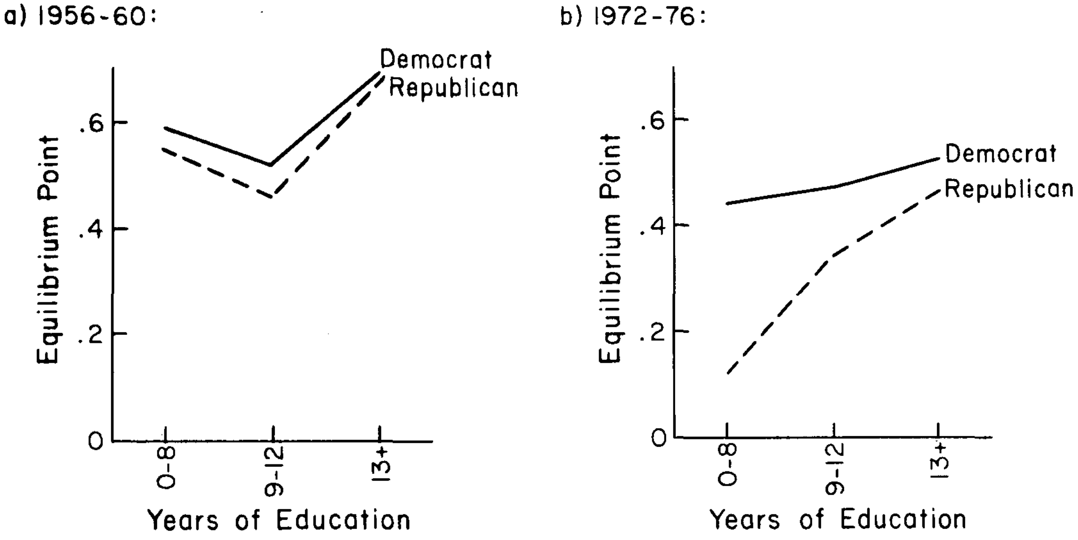
[6] To be precise, both American parties appear to be happier lodgings for the well educated than for the less educated. This could be given interpretations other than the class interest one I prefer.

[7] Over the first six CPS presidential election studies, on the average 47.1 percent of Republicans have been strong identifiers, compared with 45.5 percent for the Democrats, even though the latter were much the more popular party. Over this period, on the average 85.5 percent of the strong Democrats have voted Democratic, while 96.3 percent of strong Republican identifiers voted Republican (figures calculated from Asher, 1976).



Source: 1956–60 and 1972–76 panel studies of the national electorate conducted by the Center for Political Studies, University of Michigan.

Note: For N's and conditional probabilities, see Appendix B.

Figure 5. Equilibrium Points for Shifts between Weak and Strong Identification, U.S.A.

their attachment to either party. Of consistent partisans from 1956 to 1960, 42 percent of Republicans had more than 12 years of education, compared with 24 percent of Democrats. The comparable figures for 1972–76 are: Republicans, 54 percent; Democrats, 34 percent.

As a result, even though both panels cover periods of Republican decline, Republican partisans did not weaken in their commitment to the party as much as they might have been expected to do if the Republican party were of the same social composition as the Democratic party. Of consistent Republican partisans, there was a small net gain in percent strong from 1956 to 1960 of +.4 percent. If Republicans had had the same educational distribution as Democrats, however, they would have sustained a small net loss of –.2 percent. From 1972 to 1976, there was a sharp net loss in percent strong of –8.3 percent among consistent Republicans. If Republicans had had the same educational distribution as the Democrats, the loss would have been –11.8 percent. These are small differences, but they appear large enough to account for the small systematic advantage the Republicans have enjoyed in this regard.

Class-Voting as an Alternative to Partisanship. The fifth statement of the model based on the decisional function has a status rather different from the first four. Statements No. 1 through No. 4 set out the conditions under which individuals will need a guide to making electoral decisions. Statement No. 5 points out that where other, more efficient, guides are available, individuals will not become partisans. Such guides might include voting as one's spouse or a local political leader does, flipping a coin or whatever. One particularly interesting guide is class-consciousness. If a voter is a conscious member of a class, and if that class is represented by a particular political party, then there may be no need for the voter to develop any identification with a party per se; such voters can simply base their electoral choice on their class.

This is a relevant consideration, because it may help to explain the puzzle of European "party identification." After a flurry of enthusiasm for party identification following its successful use by Americans in the 1950s, Europeans have generally concluded that it does not help much in understanding voting behavior. It is rather unstable across time among European populations; and responses to the identification question more often seem to reflect a current voting intention than a longstanding commitment to the party. There is a suspicion that party identification in its American sense does not exist among Europeans

(Thomassen, 1975; Budge et al., 1976).

The possibility that class-voting obviates the need for a decisional guide among Europeans can provide a solution to this puzzle. In every European country there are clear class parties available to class-conscious voters; religious and other socially based parties are similarly available to other groups. Certainly, European elections and party systems have been primarily interpreted in these terms. Under these circumstances, it may not be surprising, in view of the decisional model presented here, that identification with parties per se has failed to develop among Europeans.

However, Europe may be moving into an era in which the conditions which make party identification superfluous will decline. Some party systems (notably those of Germany, France and Italy) appear to be condensing into systems with a small number of broadly based parties, which cannot make the same clear social or class appeals that parties did in the past. New issues have arisen, such as the issues surrounding European integration, which do not involve simple class or social appeals. And the social and class structure of European populations may become less simple as Europeans become more mobile. For all these reasons, the decisional function model would lead us to predict that substantial numbers of Europeans should now begin to develop party identification (Shively, 1972). In a study addressing this question, Baker (1978) provides a test of whether party identification functions more among younger generations of Germans than among older generations. He concludes that it does.

Thus, the model of the decisional function leads to an interesting explanation of the difference between American and European partisanship, with a prediction of convergence between the two. It is difficult to design tests for this interpretation, but one partial test may at least add plausibility to the suggested interpretation of European partisanship.

We have already looked at the strengthening of British partisanship. It is possible to separate out a portion of the British electorate which is relatively more "class-conscious," and compare the strengthening of partisanship among class-conscious voters to strengthening among those who are less class-conscious. The model of the decisional function predicts that for each party the relationship between education and the strengthening of party identification should be more negative among the less class-conscious than among the class-conscious. This is so because, for the class-conscious, the decisional function should be irrelevant to changes in the strength of their expressed partisanship; for the less class-conscious, however, a negative relationship between education and the strengthening of party identification, the result of that function, should be added to whatever other relationship is present.

The results of such a test are presented in Figure 6. The class-conscious have been selected by pulling together those voters who simultaneously (1) placed themselves in one of the social classes, *and* (2) chose conflict when asked, "Do you think there is bound to be some conflict between different social classes, or do you think they can get along without conflict?" All other voters were classified as "less class-conscious." By this rough measure, one-fourth of consistent Labour and Conservative partisans are class-conscious. Because of diminishing N's under this further control, I have merged the 1964–1966 and 1966–1970 panel results, and I have dichotomized education into "left school at 14 or younger," "left school at 15 or older." Even at this rate, some N's are a bit small. (See Appendix B for a report of the N's.)

We see in Figure 6 that the prediction from the model of the decisional function is borne out. For both parties, the relationship is more negative among the less class-conscious than among the more class-conscious. In the case of the Labour party, the prediction was certainly counter-intuitive, that the strengthening of Labour support should be more dependent on class for the less class-conscious voters!

Historical Variation in Electorates, and the Potential for Realignment. Since conditions associated with the decisional function have changed considerably over the last few decades in the United States, what does the model of the function imply for changing patterns of partisanship in the country? A large systematic change has occurred in levels of education, presumably implying a greater capacity to handle information. In 1900 about 70 percent of new voters were of grade-school education or less; in 1930 the comparable figure was 45 percent, and in 1970 7 percent.[8] Along with this change, one would guess that information costs have declined. Thus there should be a reduced need for guides such as party identification among the electorate.

These changes cannot have produced the large drop in partisan propensities among American voters from 1966 on, which show up

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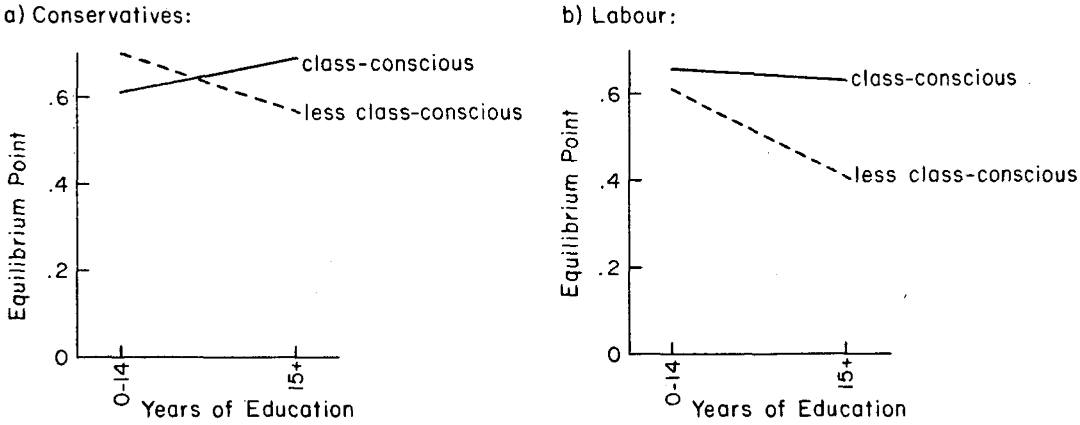
[8] 1970 United States Census. Figures for 1900 and 1930 are inferred from data in the 1940 census.



Source: 1964–66–70 panel study of the British electorate conducted by David Butler and Donald Stokes.

Note: For N's and conditional probabilities, see Appendix B.

Figure 6. Equilibrium Points for Shifts between Weak and Strong Identification, by Class-Consciousness

in Figure 2 as a fairly general decline among all groups. That drop is too sudden to have been produced by gradual demographic processes.

However, the model of the decisional function does have implications for what we might expect as a resolution of the drop in partisanship. Currently, the electorate looks ripe for realignment, according to most scenarios. Voters' sense that the electoral system is relevant and important to them has dropped, and their attachment to the present parties has dropped. In order for realignment to occur, presumably the parties should shift their positions in a way which would make them more appropriate to the political needs of the electorate; consequently, the electorate should respond by being brought back into enthusiastic participation, and then cement the new alignment by becoming "identified" with the redefined parties.

Now, according to the model presented here, different electorates will vary in how well they play out their part in this process, according to how much their members need party identification as a guide in deciding how to vote. For example, if we take the transition probabilities of the 1956–1960 panel as typical, the difference between the educational makeup of new voters in 1900 and new voters in 1970 implies that among the former under conditions of high motivation, if half the new voters were to enter as partisans, we could expect a net increase of 16 percent in the percent partisan after their first election. The comparable figure for new voters in 1970 is 9 percent. This is a hypothetical situation, but it illustrates the fact that parties have now apparently lost a good deal of their capacity to cement themselves onto the electorate through party identification.

From the model of the decisional function we would predict that an electorate like that of the 1970s, while it could call forth a redefinition of the parties and could respond to that redefinition by increased enthusiasm for elections, would *not* respond as much as past electorates have done by locking the redefined system into a pattern of party identifications. This should be a healthy change. It does not mean that political change would not happen; it means only that that change would occur more fluidly than it could do through realignments.

Conclusion

I have presented here a functional model of partisanship which yields an explanation for the neglected question, "Why do people identify with parties?" It certainly does not provide a full explanation of partisanship. Childhood acquisition of identification is not included in its purview, and there is much variation among adults in both panels examined, which is not accounted for by the model. However, even given the crude measures I have used, this model has uncovered substantial systematic adult change in the panels. The process implied in the model appears to be historically robust over the changed circumstances of the 1950s and 1970s. And it yields interesting implications for the political life-cycle, for the relationship between class and party, and for differences between European and American partisanship. I hope the model may be taken as a first step in seeking the underlying processes by which voters define their political identity.

Appendix A. Probabilities on Which Figure 1 Is Based

Listed here are $P(ID_2 \mid Ind_1)$, the conditional probability of a move from independence into identification; and $P(Ind_2 \mid ID_1)$, the conditional probability of a move from identification into independence. N's are in parentheses.

Years of Education:	Motivation High		Motivation Low	
1956–60	$P(ID_2 \mid Ind_1)$	$P(Ind_2 \mid ID_1)$	$P(ID_2 \mid Ind_1)$	$P(Ind_2 \mid ID_1)$
0–7	.62 (21)	.08 (80)	.50 (38)	.12 (97)
8			.46 (26)	.12 (83)
9–11	.48 (33)	.11 (83)	.41 (37)	.13 (92)
12	.39 (41)	.11 (105)	.53 (32)	.17 (77)
13–15	.36 (42)	.13 (119)	.40 (38)	.12 (95)
16+	.29 (14)	.12 (49)		
1972–76				
0–7	.53 (17)	.13 (63)	.32 (19)	.16 (45)
8			.27 (11)	.15 (55)
9–11	.55 (20)	.17 (53)	.55 (42)	.27 (85)
12	.26 (46)	.19 (83)	.31 (55)	.14 (84)
13–15	.28 (71)	.17 (129)	.33 (84)	.20 (79)
16+	.19 (42)	.17 (75)	.25 (32)	.15 (59)

Source: 1956–60 and 1972–76 panel studies of the national electorate conducted by the Center for Political Studies, University of Michigan.

Appendix B. Probabilities on Which Figures 4, 5, and 6 Are Based

Listed here are $P(S_2 \mid W_1)$, the conditional probability of a move from weak identification into strong identification; and $P(W_2 \mid S_1)$, the conditional probability of a move from strong identification into weak identification. N's are in parentheses.

	Britain			
	Conservatives		Labour	
	$P(S_2 \mid W_1)$	$P(W_2 \mid S_1)$	$P(S_2 \mid W_1)$	$P(W_2 \mid S_1)$
1964–66				
0–14 yrs. ed.	.37 (98)	.20 (122)	.37 (183)	.25 (199)
15	.34 (61)	.33 (40)	.26 (62)	.24 (50)
16+	.32 (59)	.29 (82)	.33 (24)	.44 (18)
1966–70				
0–14 yrs. ed.	.38 (77)	.15 (83)	.41 (120)	.21 (165)
15	.43 (42)	.27 (30)	.31 (71)	.37 (54)
16+	.44 (62)	.18 (65)	.40 (20)	.53 (19)
1964–66, 1966–70 merged				
Class-conscious				
0–14 yrs. ed.	.35 (37)	.23 (40)	.32 (79)	.17 (121)
15+	.40 (51)	.18 (43)	.33 (51)	.20 (41)
Less class-conscious				
0–14 yrs. ed.	.38 (138)	.17 (175)	.41 (224)	.26 (243)
15+	.38 (181)	.29 (166)	.29 (126)	.42 (100)

(continued on next page)

(continued)

	U.S.A.							
	Republicans				Democrats			
	$P(S_2 \mid W_1)$		$P(W_2 \mid S_1)$		$P(S_2 \mid W_1)$		$P(W_2 \mid S_1)$	
1956–60								
0–8 yrs. ed.	.29	(31)	.23	(43)	.39	(57)	.27	(78)
9–12	.35	(46)	.41	(42)	.35	(105)	.32	(105)
13+	.35	(43)	.17	(69)	.40	(55)	.18	(56)
1972–76								
0–8	.06	(18)	.42	(19)	.20	(46)	.26	(47)
9–12	.24	(34)	.46	(46)	.34	(95)	.39	(57)
13+	.29	(72)	.34	(68)	.24	(76)	.22	(54)

Source: 1964–66–70 panel study of the British electorate conducted by David Butler and Donald Stokes; 1956–60 and 1972–76 panel studies of the American electorate conducted by the Center for Political Studies, University of Michigan.

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A Dynamic Simultaneous Equation Model of Electoral Choice

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This article develops a simultaneous equation model of the voting decision in a form thought to mirror the main lines of cognitive decision-making processes of individual voters. The model goes beyond earlier efforts in two respects. First, it explicitly represents the causal interdependence of voter assessments in the election situation, permitting such estimations as the degree to which correlations between voter issue positions and issue positions ascribed to preferred candidates arise because of projection onto the candidate or persuasion by the candidate. Secondly, the model is truly dynamic, in the sense that it is dependent on longitudinal data for its proper estimation. The utility of the model is certified by the goodness of fit achieved when applied to 1972–76 panel data for a sample of the national electorate.

There is no shortage of studies which focus upon the roles of candidate personalities, partisan leanings, and contemporary issues in affecting the outcomes of elections in the United States. The value of this research is obvious, but it is probably fair to say that too much attention has been paid to the relative importance of factors idiosyncratic to particular elections and not enough effort has been directed toward the development and evaluation of an integrated and generalizable behavioral model of the voter's calculus. The result has been an uncomfortable lack of fit between verbal theories of micro-level electoral dynamics and statistical models of that process.

The goal of this study is *not* simply predictive accuracy, nor the assessment of the "relative importance" of various predictors of the vote. Nor do we intend for this work to be interpreted narrowly as a study of a particular election. What follows is a self-conscious effort at developing a model which is verisimilar to the dynamic cognitive process underlying citizens' electoral decision making. To the extent that we are able to synthesize a scattered set of verbal hypotheses and pre-theories into a precise and testable quantitative structure, we shall consider the effort a success even before confronting a shred of empirical information. Should the data corroborate the hypothesized structure or provide insights into how it might be modified—so much the better.

The model to be presented and evaluated here has two major advantages over previous work: it explicitly embodies the simultaneous interdependence of perceptions and evaluations of political stimuli specific to a particular election; and it is a truly dynamic model in that it depicts how the campaign and the ultimate vote choice modify or reinforce prior orientations. The utility of the model will be assessed by examining its goodness of fit to the 1972–1976 panel data gathered by the Center for Political Studies.

Background

Predicting the Vote. A formal model of the voter's calculus which is appealing on both normative and empirical grounds is derived from the idea of minimizing subjective expected loss (Shapiro, 1969; Davis et al., 1970; Riker and Ordeshook, 1973). The loss function is typically assumed to be quadratic, so that the loss associated with candidate j by an individual voter may be defined as:

$$L_j = (X - \theta_j)' A \, (X - \theta_j) \qquad (1)$$

where X is an mx1 vector of the voter's preferred positions on evaluation dimensions 1, 2, . . . , m; θ_j is an mx1 vector of the jth candidate's positions on these evaluation dimensions, as perceived by the voter; A is an mxm matrix of weights reflecting the relative importance of the dimensions and their covariation.

Once the expected loss associated with each candidate is assessed, the model assumes that the citizen will vote for the candidate with

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the smallest expected loss, i.e., he or she will vote "rationally." It is worth noting that, from a strictly axiomatic point of view, a choice on the basis of a candidate's personal characteristics is no less rational than one founded on a careful assessment of stated policy positions. Rationality refers to the notion of minimizing expected loss, and the model is utterly indifferent to the criteria employed by the voter in calculating these losses.

Empirically, a behavioral version of the decision rule has been shown to predict individual electoral choice very well, particularly when party identification is used as a tie-breaker in instances wherein the evaluations of the opposing candidates are similar (Kelley and Mirer, 1974; Brody and Page, 1973). Despite the predictive success of this approach, its major drawback is that it says little about the origins of candidate evaluations and thus provides a limited understanding of the broader cognitive process by which an individual arrives at a vote choice. As Brody and Page (1973, p. 10) have pointed out: "Common sense says that people probably vote for the candidate they like best." What remains is to "shift the analytical task away from an explanation of the vote to an explanation of attitudes toward candidates" (Brody and Page, 1973, p. 16). To accomplish this, the decision rule must be embedded within the context of a comprehensive model of electoral choice.

Models of Electoral Choice. In 1966, Goldberg formulated a causal system in which party identification was posited to influence candidate choice both directly and via its impact upon evaluations of parties and issues. This first attempt at modeling voting behavior represented a significant advance over earlier work, but it was flawed by methodological errors and, more importantly, by its lack of correspondence to a well-defined theory of the calculus by which an individual arrives at a candidate preference. Goldberg's causal system was recursive in structure, ruling out feedback from "partisan attitudes" to party identification, but he was nevertheless sensitive to the need to develop more realistic models which allowed for the dynamic interdependence of political orientations.

A major step in this direction was taken by Jackson (1975) when he presented a model wherein a citizen's policy preferences, partisan attachments, and evaluations of parties' positions were simultaneously related to each other and were, therefore, "endogenous to the electoral process." Testing this model against data from the 1964 Survey Research Center Election Study, Jackson found a triangular causal system operating across the three factors, with party identification influencing the voter's own issue positions, those policy preferences affecting evaluations of the parties and candidates, and they, in turn, impinging upon party identification. The vote itself was the result of an interaction between partisan identification and the evaluations of current political stimuli.

Jackson's work went far toward the development of a realistic and comprehensive model of electoral choice, but it was necessarily limited by the data sources available at the time. For instance, studies have shown that citizens' perceptions of candidate issue stands are correlated with their own policy preferences (Page and Brody, 1972), but neither Jackson's nor any other research to date has succeeded in untangling the causal nexus underlying this relationship. Is it because voters project their preferences onto the candidates, or alter their own positions in response to where they perceive the candidates to stand, or because of some other process? This question has confronted students of voting behavior since the earliest survey investigations (e.g., Berelson et al., 1954), but without suitable measures of the relevant variables—and a properly specified model—any answer has had to be largely speculative.

A second and related point is that models of electoral choice have thus far been almost exclusively static (Jackson, 1975; Schulman and Pomper, 1975; Declercq et al., 1975; Achen, 1976; Page and Jones, 1978). That is, even when cast in simultaneous-equation form, they have been obliged to focus solely on the relationships among determinants of the vote at a single point in time, leaving unanalyzed the possible ways in which prior orientations and behavior shape reactions to the stimuli of a new election, as well as the modifications produced in those orientations in turn by the new election circumstances. Plainly, the unavailability of sufficiently rich longitudinal data was a major factor in restricting models to static specifications.

Freed from many of the earlier data limitations, this study builds on the work of Jackson and others but offers some significant advances. First, the model incorporates recent thinking about the social psychological processes involved in policy-oriented voting. Second, the model is dynamic, explicitly taking into account not only the simultaneous interplay of political attitudes within the context of a single presidential campaign but also the longitudinal dependence of these attitudes from one election to the next. Among other advantages, a model in this longitudinal form can be given more satisfying specification than one tailored to synchronic measurement. This is so because useful exogenous variables are at hand in states and behaviors actually measured at earlier periods, rather than dredged up by recall. Thus we can say with considerable certainty that evaluations of Carter and Ford as presidential candidates in 1976 cannot have exerted causal influence on expressions of party identification or issue positions in 1972, and hence the latter are suitably exogenous to the nexus of attitudes in 1976. When all measurements are synchronic, it can of course be claimed that certain variables are likely to be exogenous, simply as a tactic to permit the identification of a non-recursive model. However, such arguments must always remain in the final analysis more or less suspect.[1] Our longitudinal data base substantially liberates us from this difficulty.

An Overview of the Model

The preeminence of the triology of party affiliation, issue orientations, and candidate personalities as determinants of electoral choice is firmly established in the literature, and the model to be developed here is generally in keeping with this perspective. As Figure 1 illustrates, however, we maintain that these factors are not linked directly to the vote.[2] Instead, their confluence yields a set of overall candidate evaluations, on the basis of which a choice is made. More specifically, the model posits that the citizen compares his or her summary evaluations of the candidates and votes for the one most preferred, provided the preference differential is reasonably large. However, the smaller the amount by which the voters prefers one candidate over others, the greater the influence of party loyalty in determining the final choice. For a two-candidate race at time t, the equation linking the probability of a vote for candidate 1 to candidate evaluations and party affiliation is:

$$\text{Cand.}_1\ \text{Vote}_t = a + b_1(\text{Eval.}_{1t} - \text{Eval.}_{2t}) + (b_2 - b_3 \mid \text{Eval.}_1 - \text{Eval.}_{2t} \mid) ID_t + e_t. \quad (2)$$

The first explanatory variable in Equation (2) is simply the difference in candidate evaluations, while the second term involves the absolute value of this difference and reflects the hypothesis that the impact of party ties on the individual electoral decision depends on the degree to which one candidate is preferred over the other. Note that when the evaluation differential is small, the amount by which the party identification coefficient is diminished is also small. On the other hand, if one candidate is preferred to the other by a sufficiently large margin, the coefficient for the impact of partisanship may be reduced close to zero. This aspect of the model is consonant with the spirit of the "Decision Rule" devised by Kelley and Mirer (1974) as well as with normative models of the voter's calculus. Its mathematical form is quite different, however.

It should be noted parenthetically that the model is addressed to the topic of choice among candidates and does not deal directly with the question of who votes and who does not. Implicit in this delimitation is the argument that these two concerns may indeed be fairly distinct from one another and, hence, separable for analytical purposes. This argument is made on the grounds that the decision to vote or not in a given election is determined for the most part by fairly stable attitudes toward the act of voting itself and is only secondarily affected by election-specific variables (candidates, issues, etc.). The stream of literature beginning with *The American Voter* and culminating most recently in the works of Riker and Ordeshook (1968) and Ferejohn and Fiorina (1975) supports this contention. As Campbell et al. (1960, p. 93) first put it, and as it has been demonstrated repeatedly since then, "Inquiry into the determinants of voting turnout is less a search for psychological forces that determine a decision made anew in each campaign than it is a search for the attitude correlates of voting and non-voting from which these modes of behavior have emerged and by which they are presently supported."

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[1] A common tactic with models tailored to static data is to depend on recall of earlier states for exogenous variables. Thus, for example, recall of parental partisanship may be used as an exogenous variable on grounds that current political attitudes cannot act backward causally in time to affect parental partisanship in an earlier period. When no other longitudinal information is available, such an assumption is better than nothing. However, this assumption ignores the possibility that *current* recall of earlier parental partisanship can itself be contaminated by currently evolving political attitudes.

[2] In Figure 1 we are adopting the convention that an arrow leading to another arrow denotes an interaction or mediating effect. In addition, when the simultaneous equation form of Figure 1 is presented, the coefficients and error term in each equation will be subscripted only with respect to that equation (i.e., the constant term for each equation is simply a, the error term is e_t, etc.). The purpose is to keep the notation simple, and it should cause no confusion to the reader.

Issue Orientations. The difficulty with assessing the impact of issues on the voting decision is that perceptions of candidate stands on issues of the day may vary significantly from voter to voter. Some of this variation will be unsystematic, arising from the casual attention most voters pay to campaign information—or arising from the ambiguous nature of the information itself. Against this background of noise, however, how voters perceive candidate policy positions may depend systematically upon their own issue preferences and overall affective orientations toward the candidates. For instance, voters may "project" their own issue stands onto candidates they like on other grounds, and by the same token they may tend to cognize the policy stances of negatively evaluated candidates in such a way as to increase the issue-related distance between themselves and these candidates (Page and Brody, 1972; Brody and Page, 1972).

In addition to the projection hypothesis, there is also the possibility of "persuasion." If a citizen were to alter his or her issue positions to coincide with those of a favorably regarded candidate, then one might say that the voter had been persuaded by the candidate (Brody and Page, 1972). A form of reverse persuasion would occur if a voter changed his or her position so as to contrast it with that of a negatively evaluated contender for office.

Lastly, the summary evaluations of the candidates are likely to be simultaneously interdependent with an individual's own policy preferences and perceptions of candidate positions. Specifically, the hypothesis is that, other things equal, the candidate thought to be most proximate to one's own position in the issue space will be favored.

The projection hypothesis is modeled as follows:

$$\text{Perceived Cand. Stand}_t = \text{Actual Cand. Stand}_t + b_1(R\text{'s Stand}_{t-1} - \text{Actual Cand. Stand}_t)\text{Eval.}_t + e_t. \quad (3)$$

The equation implies that a voter will cognize a candidate's policy position in such a way as to decrease the issue distance between the voter and a positively evaluated candidate or to increase the distance from a negatively evaluated candidate. Since the voter may enter the campaign period with a set of policy preferences, the respondent's issue self-placements at $t-1$ are incorporated into the equation.

The operationalization of the candidates' "actual" issue stands is problematic. One approach would involve content analysis of campaign speeches (Page and Brody, 1972). Or one might rely on the judgments of a panel of electoral experts. The method employed here is not so ambitious; we shall simply use the sample mean location of each candidate. This procedure is, of course, not without its shortcomings, and it may well be that these average placements do not correspond exactly to the candidates' "true" locations in the issue space. The mean placements nevertheless permit us to ascertain how an individual's perceived locations of the rivals for office differ from where the candidates were seen by the electorate as a whole.

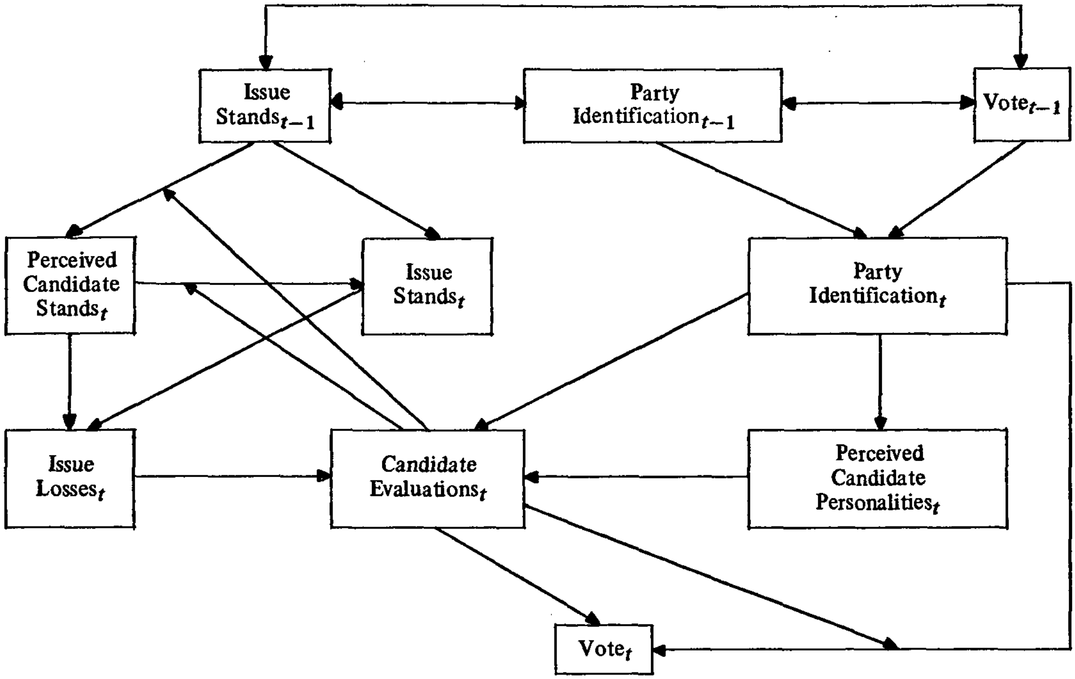
The persuasion hypothesis is modeled for a two-candidate race according to Equation (4):

$$R\text{'s Stand}_t = a + b_1\, R\text{'s Stand}_{t-1} + b_2(\text{Perceived Cand.}_1\text{ Stand}_t - R\text{'s Stand}_{t-1})\text{Eval.}_{1t} + b_3(\text{Perceived Cand.}_2\text{ Stand}_t - R\text{'s Stand}_{t-1})\text{Eval.}_{2t} + e_t. \quad (4)$$

This equation represents the idea that a voter's issue orientations prior to the campaign may be modified as a function of the candidate's issue stands, as the voter sees them. That is, the voter will move toward a favored candidate's perceived position and/or away from that of a negatively evaluated candidate.

One might question the absence of party identification in Equation (4), particularly since some recent research has suggested an increasing alignment of issue opinions and partisan ties (RePass, 1971; Pomper, 1972). However, a closer examination of the proposed model in Figure 1 reveals that the specification does not imply a lack of relationship between partisanship and issue opinions. First, the *long-term* effect of party affiliation on policy preferences is captured in their correlation at $t-1$. In addition, the *endogenous* influence of party ties on issue stances is posited to flow via the effect of such ties on candidate evaluations.

This specification is supported both empirically (Markus, 1976; Converse and Markus, 1979) and by theoretical considerations. With regard to the latter, the argument is that, within the course of a presidential campaign, issues become imbued with partisan qualities insofar as the partisan rivals for office come to be identified with particular policies. The candidates form the link between partisanship and


Issue Stands$_{t-1}$
Party Identification$_{t-1}$
Vote$_{t-1}$
Perceived Candidate Stands$_t$
Issue Stands$_t$
Party Identification$_t$
Issue Losses$_t$
Candidate Evaluations$_t$
Perceived Candidate Personalities$_t$
Vote$_t$

Source: Compiled by the authors.

Figure 1. A Model of the Voting Decision

intra-campaign change in policy-related attitudes.

Equation (5) is derived from the ideas underlying the loss function model of evaluation. It posits that the net candidate evaluation differential is a function of issue-based losses and losses (or, in the positive, gains) accruing from preferences in candidate personalities and partisan identification:

$$(\text{Eval.}_{1t} - \text{Eval.}_{2t}) = a + b_1(\text{Loss}_{1t} - \text{Loss}_{2t}) + b_2(\text{Personality}_{1t} - \text{Personality}_{2t}) + b_3 ID_t + e_t. \quad (5)$$

Candidate Personality. One would expect that, other things equal, Democrats will tend to prefer the personal traits of a Democratic candidate to those of a Republican office-seeker, and conversely for Republicans. The model, therefore, includes an equation which posits the difference in a citizen's ratings of the candidates' personalities to be a function of partisan ties:

$$(\text{Personality}_{1t} - \text{Personality}_{2t}) = a + b_1 ID_t + e_t. \quad (6)$$

Party Identification. The party identification equation is structured to reflect the possibility that while partisanship is relatively stable through time, it may nonetheless be influenced by prior voting behavior. Thus although the vote is the ultimate dependent variable within the context of a single presidential election, from a dynamic perspective it may feed forward to influence the voter's future political orientations.[3] The model also permits the precursors of current partisanship to interact, with voters and nonvoters differing in terms of the relative stability of party identification through time:

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[3] It is important to keep in mind here, as elsewhere in the model equations, that we are less making an assertive assumption that prior votes *do* influence party identification, than merely building a model structure which permits such effects to be discriminated *if they exist.* Earlier recursive models of the Goldberg (1966) type were obliged to proclaim by assumption that these reverse effects of vote on party identification did not exist, at least within the time frame of the model. We do not make a contrary proclamation that such effects *do* exist, since a given testing of the model in a special case might show the relevant coefficients to be zero.

$$\begin{aligned} ID_t = a &+ b_1 \text{Demo. Vote}_{t-1} + \\ &b_2 \text{Repub. Vote}_{t-1} + (b_3 + \\ &b_4 \text{Voted}_{t-1}) ID_{t-1} + e_t. \end{aligned} \qquad (7)$$

Results

As mentioned earlier, the model of electoral choice was fitted against the CPS 1972–1976 panel data. These data contain information on a national sample of 1286 adult respondents interviewed in the periods immediately following the two presidential elections (see Miller et al., 1976; Miller and Miller, 1977; Converse and Markus, 1979).

Before detailing the results, we should make a few technical comments about our analytical methods. Because of the simultaneous nature of the system of equations, we employed two-stage least squares as an estimator, except in those instances where ordinary least squares estimation appeared justified. Lagged dependent variables were treated as predetermined with respect to the 1976 data, implying a lack of autocorrelation in the disturbances. Although this assumption is perhaps not fully warranted, it is not unreasonable, given the fairly long time lag between waves (cf. Hibbs, 1972). The assumption also finds support in some of our earlier analyses (Converse and Markus, 1979). Finally, the estimation proceeded in modular fashion from equation to equation, rather than being carried out initially for the structure as a whole, and thereby avoids the considerable attrition of case numbers that occurs when cases with missing data on as little as one of the totality of variables in the structure are deleted from any role in the estimation.

Party Identification. The party identification variable ranges in seven integer steps from −3 (Strong Democrat) to +3 (Strong Republican). For purposes of operationalization, the other variables in Equation (7) are coded in a binary fashion, denoting whether the respondent voted in 1972 and, if so, for the candidate of which party. Nonvoters are coded zero on all three binary variables.

Estimation of Equation (7) by ordinary least squares yields the following results (standard errors in parentheses):

$$\begin{aligned} ID_t = &\underset{(.07)}{-.37} - \underset{(.10)}{.20} \text{ Demo. Vote}_{t-1} + \\ &\underset{(.09)}{.49} \text{ Repub. Vote}_{t-1} + \\ &(\underset{(.04)}{.63} + \underset{(.05)}{.10} \text{ Voted}_{t-1}) ID_{t-1} + e_t. \end{aligned}$$

$$R^2 = .64; \quad N = 1252.$$

Party identification is found to be very durable from one election to the next, a result consistent with work on these and other data. Also, as hypothesized, the relationship between prior and current partisanship is somewhat stronger among voters than among nonvoters (with an estimated coefficient of .63 + .10 = .73 for voters versus .63 for nonvoters in 1972). But while partisan attachments move relatively little from one election to the next, they are not completely immune to electoral forces, as demonstrated by the significant coefficients attached to the partisan voting variables.

The feedforward effect of electoral choice on party identification is displayed in Table 1,

Table 1. Predictions of Party Identification$_t$, by Prior Partisanship and Voting*

	Party ID$_{t-1}$						
Vote$_{t-1}$	Strong Democrat	Democrat	Independent Democrat	Independent	Independent Republican	Republican	Strong Republican
Nixon	−2.07 (D)	−1.34 (ID)	− .61 (ID)	.12 (I)	.85 (IR)	1.58 (R)	2.31 (R)
None	−2.25 (D)	−1.63 (D)	−1.00 (ID)	−.37 (I)	.26 (I)	.88 (IR)	1.51 (R)
McGovern	−2.76 (SD)	−2.04 (D)	−1.31 (ID)	−.58 (ID)	.15 (I)	.88 (IR)	1.61 (R)

Source: Equation (7), as estimated using data from the 1972–76 Center for Political Studies National Election Panel Study.

*Mid-points between scale values were used as cut-points to generate the parenthesized category predictions.

in which the coefficient estimates have been used to generate predictions of party identification at time *t* for different combinations of past identification and vote. The table shows that when partisanship is not reinforced by consistent electoral behavior, it may weaken. The predicted effect of a deviating vote in a single election is hardly dramatic; indeed, if it were, there would be reason to doubt the analysis. Nevertheless, the cumulative effects of a series of votes running counter to an individual's prior party ties might well lead to a conversion of partisan orientations at the individual level.

Issue Opinions. Five policy domains were selected for study here: social welfare, busing, government assistance to minority groups, tax reform, and women's rights. These issues were chosen because comparable items dealing with them were included in both the 1972 and 1976 Election Studies and because they were issues which were salient to many voters in 1976, as will be illustrated in a moment. For each policy domain, respondents were asked to place themselves and the major party candidates along a seven-point continuum with labeled endpoints.[4]

It is, of course, possible that the inclusion of other issues to the set of five might have altered the results of the analysis significantly, but we deem this unlikely. For one thing, the five issue domains appear to cover most of the public's major policy concerns in 1976. Our evidence for this statement is derived from an open-ended query in the 1976 interview regarding "the most important problem the country faces." Some degree of ambiguity is always inherent in mapping open-ended responses into fixed categories, but by our count over three-quarters of the answers directly concerned one or more of the five issues under study.

Perceptions of where the presidential candidates stood on the issues in 1976 varied with an individual's own policy preferences and affective orientations toward the contenders (see Table 2). Generally, a citizen's placement of a favorably evaluated candidate along an issue continuum is positively associated with the individual's own preferred position on the seven-point scale. At the same time, the perceived location of a negatively evaluated candidate is inversely correlated with self-placement. This relationship is by no means invariant across issue domains and candidates, nor do average perceptions of candidate positions float unanchored across the entire seven-point range. Nevertheless, the tabular results display an unmistakable regularity. Table 2 does not, however, enable one to gauge the extent of persuasion or projection, since the pattern of association is consistent with either of these hypotheses—or a combination of them. We must turn to the simultaneous equation model for further explanation of the interdependencies exhibited above.

Table 3 presents the two-stage least squares estimates of the ten (two candidates x five issues) "projection" equations. The R^2 values associated with these equations are fairly low, but then this is not unexpected: probably most of the variation in perceptions of candidate stands is simply noise arising from the presidential office-seekers' strategy of obscuring their positions on specific issues—especially in 1976.[5] Nevertheless, in all instances save one, the regression coefficient reflecting the extent of projection is significant, hovering around an estimated value of .005. One way of interpreting this value is that it implies that for a very favorably evaluated candidate (i.e., one receiving a translated thermometer score near +50), up to 25 percent of any difference between the candidate's objective issue position and the respondent's favored position would be "projected away."[6] Another way of interpreting these estimates is illustrated in Table 4. In that table, predicted candidate placements have been calculated for various combinations of self-placements on the first two issues and feelings toward the candidates. By comparing these predictions with the actual mean scores in Table 2, one can see that the mathematically parsimonious projection equations yield aggregate profiles that are consistent with the observed findings.

The two-stage least squares estimates of the equations for voters' preferences on the five

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[4] The text of the issue items may be found in the CPS 1976 National Election Study codebook, available from the Inter-University Consortium for Political and Social Research.

[5] Correlations are also probably depressed by the presence of error arising from imprecise instrumentation and the stochastic nature of the latent attitudes being measured (Converse, 1970; Achen, 1975; Converse and Markus, 1979).

[6] Multiplication of the .005 average coefficient estimate by the thermometer score of 50 yields the 25 percent figure. The translated thermometer used in estimating Equations (3) and (4) ranges from −50 to +50 degrees.

Table 2. Mean Candidate Issue Placement, by Self-Placement and Candidate Feeling Thermometer Score*

	Social Welfare							
	Carter				Ford			
R's Location	Warm		Cool		Warm		Cool	
1 (Liberal)	2.2	(77)	4.8	(4)	3.4	(31)	6.2	(40)
2	2.5	(48)	3.9	(7)	3.6	(35)	5.9	(21)
3	2.8	(96)	3.4	(18)	3.9	(81)	5.4	(34)
4	3.2	(137)	3.0	(42)	4.1	(149)	5.3	(36)
5	3.8	(103)	2.5	(56)	4.4	(123)	5.0	(32)
6	3.5	(77)	2.2	(48)	4.5	(105)	4.2	(15)
7 (Conservative)	3.7	(87)	2.6	(77)	4.7	(142)	3.7	(28)
Grand Mean	3.2	(625)	2.6	(252)	4.3	(666)	5.2	(206)
eta-squared	.15		.13		.07		.27	

	Busing							
	Carter				Ford			
R's Location	Warm		Cool		Warm		Cool	
1 (Favor)	3.3	(28)	5.0	(3)	2.5	(10)	6.0	(20)
2	2.7	(27)	5.0	(2)	4.1	(21)	6.1	(8)
3	3.4	(18)	5.0	(2)	4.0	(16)	5.7	(6)
4	3.8	(44)	5.2	(12)	4.1	(39)	5.1	(19)
5	4.1	(32)	3.0	(11)	4.4	(36)	4.4	(14)
6	4.2	(79)	3.5	(35)	4.2	(105)	3.8	(22)
7 (Oppose)	4.2	(318)	3.3	(137)	4.5	(441)	3.3	(120)
Grand Mean	4.0	(546)	3.4	(202)	4.4	(668)	4.0	(209)
eta-squared	.06		.05		.03		.21	

	Tax Reform							
	Carter				Ford			
R's Location	Warm		Cool		Warm		Cool	
1 (Progressive)	2.6	(109)	3.9	(17)	4.0	(70)	5.3	(44)
2	2.8	(64)	2.9	(20)	3.6	(58)	5.0	(23)
3	3.1	(78)	3.0	(25)	3.9	(77)	4.5	(18)
4	3.3	(96)	2.8	(36)	4.2	(110)	4.4	(25)
5	3.5	(44)	2.8	(18)	4.6	(60)	3.9	(9)
6	4.0	(36)	3.0	(26)	4.5	(46)	4.9	(14)
7 (Same Rate)	4.3	(13)	3.4	(51)	5.0	(133)	4.2	(39)
Grand Mean	3.4	(557)	3.1	(193)	4.3	(554)	4.7	(172)
eta-squared	.17		.04		.10		.07	

	Minority Groups							
	Carter				Ford			
R's Location	Warm		Cool		Warm		Cool	
1 (Government Help)	2.3	(69)	4.0	(7)	2.9	(44)	5.8	(35)
2	2.5	(52)	3.0	(10)	3.3	(45)	4.8	(18)
3	2.9	(102)	3.0	(34)	3.4	(116)	4.6	(22)
4	3.4	(128)	3.0	(41)	3.8	(138)	4.4	(41)
5	3.6	(77)	3.3	(51)	4.3	(112)	4.5	(22)
6	3.8	(73)	3.1	(41)	4.1	(92)	3.3	(22)
7	4.2	(81)	2.4	(60)	4.5	(110)	3.0	(35)
Grand Mean	3.3	(582)	3.0	(244)	3.9	(657)	4.3	(195)
eta-squared	.20		.06		.12		.29	

Table 2 (continued)

	Women's Rights							
	Carter				Ford			
R's Location	Warm		Cool		Warm		Cool	
1 (Equal Rights)	2.6	(194)	2.5	(58)	2.3	(181)	3.6	(77)
2	3.0	(83)	3.2	(27)	3.0	(95)	4.1	(21)
3	3.3	(51)	3.6	(18)	3.5	(60)	4.1	(15)
4	3.5	(103)	3.5	(38)	3.7	(122)	4.5	(28)
5	3.9	(43)	3.5	(21)	4.0	(54)	3.6	(14)
6	4.1	(34)	3.0	(11)	4.0	(37)	2.8	(6)
7	4.1	(59)	3.7	(7)	4.0	(48)	2.9	(14)
Grand Mean	3.2	(567)	3.1	(180)	3.2	(597)	3.8	(175)
eta-squared	.16		.12		.23		.09	

Source: 1976 Center for Political Studies National Election Panel Study.

*Cool feelings include feeling thermometer scores of 0–49 degrees. Warm feelings are scores of 51–100 degrees.

issues are presented in Table 5. The analysis indicates that issue opinions possess a durability which, although much less than that of party identification, is nonetheless quite significant.[7] There is also evidence of the persuasion or leadership effect with regard to three policy domains: welfare, minority groups, and tax reform. The presidential candidates apparently exerted very little influence upon public sentiments towards busing and women's rights, however.

These findings recall a pattern that emerged in our first cut at these data (Converse and Markus, 1979). In that study, the stability of opinion on various issues was found to be arrayed hierarchically, with attitudes on issues of a moral nature displaying a distinctively higher degree of temporal stability and hence apparently greater crystallization than did opinions on civil rights, domestic policy, and foreign affairs. It stands to reason that attitudes tied closely to one's sense of morality would not be very susceptible to the influence of campaign debate, and that is precisely what we find here.

Lastly, since the dependent and persuasion variables from Table 5 are expressed in the same currencies as the dependent and regressor variables, respectively, in the preceding set of equations, the relative effects of projection and persuasion may be compared.[8] Upon doing so,

Table 3. Two-Stage Least Square Estimates for the Perceived Candidate Issue Position Equations

Issue	Candidate	Mean	Coefficient	Standard Error	N	R^2
Welfare	Carter	3.01	.007	.001	854	.08
	Ford	4.51	.006	.001	869	.02
Busing	Carter	3.84	.005	.001	701	.06
	Ford	4.26	.007	.001	848	.06
Minorities	Carter	3.18	.005	.001	773	.04
	Ford	4.00	.005	.001	813	.04
Tax Reform	Carter	3.32	.000	.000	735	.00
	Ford	4.40	.003	.001	685	.01
Women	Carter	3.20	.002	.001	711	.01
	Ford	3.37	.005	.001	748	.03

Source: 1972–76 Center for Political Studies Election Panel Study.

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[7] See Converse and Markus (1979) for a further discussion of the relative stabilities of political outlooks.

[8] We prefer the unstandardized coefficients to standardized ones for two reasons. First, the equations are nonlinear in the variables, rendering the usual

Table 4. Predicted Perceived Candidate Location for Various Combinations of Self-Placement and Candidate Feeling Thermometer Score*

Respondent Position	Social Welfare			
	Carter		Ford	
	Warm	Cool	Warm	Cool
1 (Liberal)	2.7	3.4	4.0	5.0
3	3.0	3.0	4.3	4.7
5	3.4	2.7	4.6	4.4
7 (Conservative)	3.7	2.3	4.9	4.1

Respondent Position	Busing			
	Carter		Ford	
	Warm	Cool	Warm	Cool
1 (Favor)	3.5	4.2	3.7	4.8
3	3.7	3.9	4.0	4.5
5	4.0	3.7	4.4	4.1
7 (Oppose)	4.2	3.5	4.7	3.8

Source: Equation (3), as estimated using data from the 1972–76 Center for Political Studies National Election Panel Study.

*Scores of −25 and +25 degrees were used to represent cool and warm feelings, respectively, on a translated feeling thermometer ranging from −50 to +50 degrees. The former values are virtually identical to the observed mean feeling scores for the two sets of respondents.

one finds that with respect to both Carter and Ford the projection coefficients are larger than the corresponding persuasion values for all issues but one. The differences are by no means staggering, but they do suggest that projection is about 20 percent stronger than persuasion, on the average. One should bear in mind, however, that under certain circumstances—e.g., a new issue for which public opinion has not yet jelled—the influence of political leaders upon the electorate's policy preferences might be substantial.

Candidate Personalities. In 1976, respondents were asked to rate on a seven-point scale the degree to which each of the major candidates had "the kind of personality a President ought to have." The estimated equation linking the difference in ratings of Ford's and Carter's personalities to partisanship is (standard errors in parentheses):[9]

$$(\text{Ford Personality} - \text{Carter Personality}) = \underset{(.07)}{.20} + \underset{(.04)}{.60}\, ID_t + e_t.$$

The equation accounts for 20 percent of the variation in the dependent variable (N = 1193), equivalent to a standardized regression coefficient of .44 for party identification. On the average, the personalities of both candidates were scored at about 1.4 on a −3 to +3 scale. The unstandardized coefficient estimates show Ford receiving a score about two points higher than Carter among Strong Republicans, however, and about 1.6 points below Carter among Strong Democrats.[10]

Evaluations of the Candidates. Equation (5) posits that a citizen's overall evaluations of the presidential candidates are formed from a mix of policy considerations, partisan predisposi-

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interpretation of standardized coefficients may be arbitrarily altered by rescaling the original variables (Allison, 1977). Moreover, since standardized coefficients are by definition variance-sensitive, comparisons of their values across equations—even linear ones—can lead to misinterpretations.

[9] The equation was estimated by ordinary least squares, which assumes a lack of correlation between the disturbances in the party identification and personality differential equations. The consequence of this assumption being incorrect would be to overstate the impact of the independent variable. Given the plausibly moderate value of the coefficient estimate, the assumption appears not to have been grossly unreasonable.

[10] The predicted dependent variable value is .20 + .60(+3) = 2.0 for Strong Republicans and .20 + .60(−3) = 1.6 for Strong Democrats.

Table 5. Two-Stage Least Squares Estimates for the Voter Issue Position Equation

	Coefficient	Standard Error
Social Welfare		
Constant	2.59	.14
Issue Position$_{t-1}$	.47	.03
Carter Persuasion	.006	.001
Ford Persuasion	.006	.002
$R^2 = .29$ N = 883		
Busing		
Constant	2.35	.19
Issue Position$_{t-1}$	.61	.03
Carter Persuasion	.002	.001
Ford Persuasion	.003	.001
$R^2 = .40$ N = 685		
Minority Groups		
Constant	2.25	.14
Issue Position$_{t-1}$	.52	.03
Carter Persuasion	.004	.001
Ford Persuasion	.004	.002
$R^2 = .33$ N = 764		
Tax Reform		
Constant	2.54	.22
Issue Position$_{t-1}$	.40	.05
Carter Persuasion	.007	.002
Ford Persuasion	.004	.002
$R^2 = .16$ N = 657		
Women's Rights		
Constant	1.67	.16
Issue Position$_{t-1}$	.41	.05
Carter Persuasion	−.001	.002
Ford Persuasion	.001	.003
$R^2 = .21$ N = 689		

Source: Data from the 1972–76 Center for Political Studies National Election Panel Study.

tions, and beliefs about the personalities of the rivals for office. The operationalizations of the regressors in that equation are by now familiar, with the exception of the issue-loss component. The issue loss associated with each candidate was constructed as the average squared difference between the respondent's self-placement on each issue and his or her perceived location of the candidate. We used an average rather than a simple sum of squares because not all respondents could provide both a preferred policy and a candidate location for all five issues.

This method weights each included issue equally, a procedure which appears to conflict with the notion that all issues may not be equally salient to the voter. A number of considerations render this approach less objectionable than it might seem, however. First, taken as a whole the five issues are salient ones, and the majority of respondents were able to locate themselves and the candidates on each of the issue continua. Moreover, issues that are utterly non-salient to a respondent are given a weight of zero, since both self and candidate placements are required for the issue to be included in the loss calculus. Third, preliminary attempts to devise a weighting scheme based on open-ended and other responses actually led to a slight decrease in explanatory power for the regression equation. This came as little surprise, since a number of recent psychological studies have demonstrated that people are generally quite unreliable in assessing the relative importance of factors in determining their decisions (Nisbett and Wilson, 1977). Moreover, other work indicates that the choice of weights tends to make little difference in the ultimate predictions (Wainer, 1976; Dawes and Corrigan, 1974).

The two-stage least squares estimates for the candidate evaluation equation are displayed in Table 6. The analysis indicates that all three elements—issues, party, and personalities—were important determinants of feelings toward Ford and Carter. Taken together, these three variables account for two-thirds of the variation in the feeling thermometer differential for the presidential rivals.

The results suggest that the perceived personal qualities of the candidates weighed most heavily, at least in the direct-effect sense, in determining the public's overall evaluations of Ford and Carter. The standardized coefficient for the personality differential variable equals .43, and the unstandardized coefficient implies nearly a 40-point difference (6.26 x 6) in the thermometer scores of candidates rated at opposite extremes of the presidential personality scale.

Issues and party ties were about equally important in terms of their direct effect on candidate evaluations, with standardized coefficients of –.30 and .29, respectively. Keeping in mind that the issue loss differential can range from +36 (favoring Carter) to –36 (favoring Ford), note that the unstandardized coefficient for that variable implies that for each unit increase in Ford's loss value relative to Carter's, the voter's thermometer difference (Ford–Carter) decreased by a little more than one degree, other variables held constant. At the same time, the coefficient for party identification indicates a 20-degree differential favoring Ford among Strong Republicans and a 14-degree contrast

Table 6. Two-Stage Least Squares Estimates for the Evaluation Differential Equation

	Unstandardized Coefficient	Standard Error	Standardized Coefficient
Constant	2.72	.77	
Ford–Carter Issue Loss	–1.34	.38	–.30
Ford–Carter Personality	6.26	.51	.43
ID_t	5.66	.64	.29
$R^2 = .67$ N = 1013			

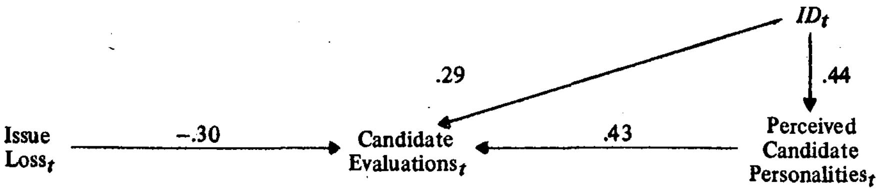
Source: Data from the 1972–76 Center for Political Studies National Election Panel Study.

favoring Carter among Strong Democrats, *ceteris paribus.*[11]

The fairly modest standardized coefficient for party identification might be somewhat puzzling, given the crucial role that partisanship has been assumed to play in electoral behavior. It is important to remember, however, that in addition to its direct impact on candidate evaluations, party affiliation also exerts a substantial *indirect* influence via its effect upon perceptions of candidate personalities. When this indirect influence is combined with the direct effect, it yields an "effects" coefficient of .29 + .43(.44) = .48, larger than the standardized coefficient for either candidate personalities or issue proximities (see Figure 2). This latter value may be more in line with intuitive judgments–and prior evidence–about the impact of partisanship on candidate evaluations. Moreover, under some circumstances party identification may play a significant role in determining the electoral decision quite apart from its influence on feelings toward the candidates, as we shall see in a moment.

The Vote Decision. Least squares estimation of the vote choice equation generates the following results (standard errors in parentheses):[12]

$$\text{Ford Vote} = \underset{(.010)}{.506} + \underset{(.0004)}{.009}\,(\text{Ford Eval.} - \text{Carter Eval.}) + (\underset{(.009)}{.141} - \underset{(.0002)}{.0025}\,|\text{Ford Eval.} - \text{Carter Eval.}|)ID_t + e_t.$$


ID_t
.29
.44
Issue Loss$_t$
–.30
Candidate Evaluations$_t$
.43
Perceived Candidate Personalities$_t$

Source: Based on the regression estimates outlined in the text.

Figure 2. Candidate Evaluations Segment of the Model of Electoral Choice

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[11] For Strong Republicans, the expected thermometer differential is 2.72 + 5.66(+3) = 19.70; for Strong Democrats it is 2.72 + 5.66 (–3) = –14.26.

[12] Both two-stage least squares (2SLS) and ordinary least squares (OLS) estimators were originally employed. Although the methods yielded similar coefficient estimates, the OLS values are preferred here because they yield fewer predictions outside of the allowable 0–1 range and because the 2SLS residuals were only modestly correlated with residuals elsewhere in the system, thus permitting the more efficient OLS estimation. The 2SLS estimates (with standard errors parenthesized) are:

$$\text{Ford Vote} = \underset{(.012)}{.503} + \underset{(.001)}{.011}\,(\text{Ford Eval.} - \text{Carter Eval.}) + (\underset{(.019)}{.157} - \underset{(.0007)}{.0036}\,|\text{Ford Eval.} - \text{Carter Eval.}|)ID_t + e_t.$$

$R^2 = .65$; N = 753.

The regression analysis lends clear support to the hypothesized vote choice equation. All coefficients have the expected signs and are many times larger than their associated standard errors. The R^2 value for the equation equals .64 (N = 884), but because of the dichotomous nature of the dependent variable, the best measure of goodness of fit is provided by the fact that the equation correctly predicts respondents' votes 90 percent of the time.[13]

Moreover, it can be shown that the direct intrusion of party identification in influencing the vote choice when there is relative indifference in candidate evaluations is the only case in which prior variables in the model affect the vote, save indirectly through the comparative candidate evaluations. That is, the addition of other model variables, including most notably the issue losses and personality ratings, to the simple vote choice regression equation based on differential candidate evaluations and party identification, leads to virtually no increase in predictive accuracy. As a further test, we applied the estimated equation to the data from the 1972 wave of the panel to ascertain its predictive accuracy with regard to the Nixon-McGovern contest.[14] The result was that 97 percent of the voters were correctly classified, a very satisfactory result given that the coefficient estimates are based on an entirely different set of data. Indeed, the predictive accuracy might seem to be *too* good, but the explanation is simply that evaluations of Nixon and McGovern tended to be more disparate than those for Ford and Carter, and hence electoral choice was more predictable than in 1976.

The values of the coefficient estimates possess some charming qualities. For instance, the constant term implies a virtual 50–50 split in the vote of Independents who evaluated the two candidates equally. Furthermore, if one simulates an election wherein net short-term partisan forces (i.e., the evaluation differential) are zero and inserts the sample mean for the party identification variable, the prediction (interpreting the result in aggregate terms) is a 46 percent vote for the Republican candidate—precisely the value of the normal vote (Converse, 1966).

Note that the direct impact of party identification upon the vote decision depends upon the size of the candidate evaluation differential. When both candidates are evaluated identically, each step toward the Republican end of the partisan continuum increases the predicted dependent score (roughly, the probability of voting for Ford) by .14. Once the difference in candidate evaluations reaches about 55 degrees, however, the predicted direct effect is partisanship on the vote is virtually nil, as indicated by its small coefficient: .141 – .0025(55) = .003.[15] Although the direct influence of party ties on the vote decision would be negligible under these circumstances, their indirect effect, i.e., their prior impact on the candidate evaluations themselves, nevertheless remains appreciable.

The relative effects of candidate evaluations and party identification on the choice between presidential contenders is illustrated in Table 7. As displayed there, the predicted probability of a Ford vote ranges from .00 for Strong Democrats, with a 50 degree evaluation differential favoring Carter, to 1.00 for Strong Republicans, with an equally large pro-Ford difference. The table also illustrates the varying impact of party identification, depending on the contrast in candidate evaluations: when the differential is large, the probability entries do not vary much across partisan categories; if the evaluation differential is within the bounds ±20 degrees, however, the residual impact of partisanship on electoral choice is more potent.

Conclusions

From a substantive point of view, a model is something more than the sum of its parts. We may therefore conclude by moving from an examination of the separate equations comprising the model to a brief summary and discussion of its broader implications.

Perhaps most importantly, the hypothesized model of electoral choice has fared rather well against the 1972–1976 panel data. No alterations in the posited specification were indicated. To the contrary, a number of R^2 values approach what must be their upper bounds,

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[13] Respondents with estimated dependent variable scores >.50 were predicted to have voted for Ford, while those with estimated values ≤.50 were classed as Carter voters.

[14] This prediction to the 1972 presidential vote refers, of course, to the vote choice equation taken alone. The full model being presented could not be estimated for 1972 separately, for lack of prior panel data in that year.

[15] In 1976, 15 percent of all respondents saw no difference in their overall evaluations of the candidates. One half of the sample had evaluation differentials of 25 degrees or less, and nearly 90 percent are included in the ±55 degree span.

Table 7. Predicted Probability of a Ford Vote, by Party Identification and Candidate Evaluation Difference

	Party Identification						
Differential	Strong Democrat	Democrat	Independent Democrat	Independent	Independent Republican	Republican	Strong Republican
(Pro-Carter) −50	.00	.02	.04	.06	.07	.09	.10
−40	.02	.06	.11	.15	.19	.23	.27
−20	.05	.14	.24	.33	.42	.51	.60
−10	.07	.18	.30	.42	.53	.65	.76
0	.08	.22	.37	.51	.65	.79	.93
10	.25	.36	.48	.60	.71	.83	.94
20	.41	.50	.60	.69	.78	.87	.96
40	.74	.78	.83	.87	.91	.95	.99
(Pro-Ford) 50	.91	.92	.94	.96	.97	.99	1.00

Source: Equation (2), as estimated using the 1972–76 Center for Political Studise National Election Panel Study.

given normal sampling error and other idiosyncratic sources of noise.

The analysis affirms the crucial role of candidates in the dynamics of electoral choice. Candidate evaluations have been shown to be a primary determinant of the vote, with policy considerations and even partisan orientations affecting the vote either exclusively or largely through the way they help to shape feelings toward the presidential rivals. The candidates also mediate the flow of campaign stimuli toward the cognitive predispositions comprising the citizen's network of political beliefs: they serve as the primary vehicle through which policy debate takes on partisan coloration, and through their determining influence on the vote, feelings toward the candidates even act dynamically in at least a small degree to modify or reinforce standing party ties. In the American system of elections, the choice is ultimately between competing candidates.

The fact that comparative candidate assessments are the most potent proximal determinant of the vote decision should not, however, lead us to overlook the causally prior impacts of issues and parties on these assessments. Policy considerations were shown to be significant in determining voters' evaluations of the 1976 presidential candidates. Some of these manifest linkages between issues and candidates turned out to be modestly circular in one sense or another. Thus, for example, a degree of "projection" was uncovered, whereby voters reporting issue positions of candidates they already liked would shade these perceptions toward the issue positions the voter already preferred, while assuming that disliked candidates must have more sharply dissimilar positions. In the same causal nexus it was possible to isolate a persuasion effect as well, whereby the voter appeared to be shifting his or her own reported issue position to conform more closely with that of a preferred candidate, or to distinguish it more sharply from that perceived for a disliked candidate. The persuasion effect was less strong in these data than the projection effect, and furthermore showed signs of variation by type of issue. For issues like school busing or women's rights, where there is independent evidence of sharp crystallization of public feeling, any persuasion effect is negligible. It is chiefly on issues where voters are less personally exercised that the policy position of candidates can sway their admirers' views of the issue.

However intriguing and plausible these side effects may be, the estimates we have derived from the model make clear that they remained no more than side effects in 1976. The policy differences consensually perceived to exist between the candidates, coupled with prior differences in voter positions on these issues, had a noteworthy effect on voters' comparative assessments of the candidates, and through these invidious assessments, the policy terms ultimately left their mark on final voting decisions.

Similarly, the model helps to delineate more clearly than did prior work the important function of partisan predispositions in the processes leading to a voting choice. Earlier investigations limited to static data bases have often tried to assess the relative role of parties, issues and candidates by assigning each a single ultimate regression weight, leading to comparisons which take on the flavor of a simplistic horse race. Viewing the formulation of a voting decision in terms which are more explicitly processual may rob us of such a simple "final score," but may yield redeeming satisfactions due to greater verisimilitude.

In such process terms, the causal role of partisanship appears to be particularly important for two reasons. First, party identifications are much more stable in the intermediate term than other elements in the model. If the game were redefined as one of predicting a voting decision on the basis of political attitudes examined eight years before election day, there would be little contest: the identity of the candidates would be utterly unknown at such remove, some issues to become important later would also be unknown, and voter attitudes on other more abiding issues would be subject to considerably greater flux in the interim.

The second reason shows up clearly in the structure of the model and hinges on the fact that the party component is unique in the way it intrudes at multiple points in the process. Partisan predispositions may be outweighed by other model terms at particular stages, as other research on a static base has shown, but these loyalties keep coming back as determinants while the vote decision process unrolls. For example, Figure 2 suggested that neither issues nor partisanship were as important in their direct effects upon ultimate comparisons between the candidates as were simple judgments of their personalities. However, the very same figure makes clear that reactions to candidate personalities have already been shaped by prior partisan predispositions, so that there is an indirect path from party to candidate evaluations which is of imposing proportions in itself, quite apart from the direct path also depicted. Or at a later point, partisanship again enters the model significantly in influencing the final vote when the voter has trouble making sharp affective discriminations between the candidates.

In the same vein, given the durable nature of these loyalties over lengthy periods of time (Converse and Markus, 1979), it may well be that prior partisanship has also intruded at stages too early for adequate representation in our model. Thus, for example, we located an edge of "persuasion" whereby the voters' issue locations were mildly affected by the issue positions perceived for liked and disliked candidates. If such candidate persuasion exists, then it would not be at all surprising if there were an earlier *party* persuasion term of entirely parallel structure, accounting for some of the party-relatedness of issue positions at our first time point, yet which is exogenous to our model as presented. Such party persuasion would presumably be operative in the first stages of issue emergence, and hence likely to have more impact on the distribution of attitudes in the electorate than do candidates who arrive on the scene after such issue positions have become at least modestly crystallized.

In short, then, while partisan predispositions are unlikely to dominate the process completely at given stages where the candidates are being assessed, these loyalties appear to make repeated inputs of substantial magnitude throughout the process.

We believe that the model as presented is an integrated and generalizable one that captures most of what is important as voters face elections involving candidate competition. The model could be enlarged to become more general still. It could, for example, add considerations of turnout, representing the fact that voting and nonvoting are for the most part habitual differences, but capturing those margins of turnout variance that are in fact current and dynamic responses to the specific election configuration, such as the likely diminution in turnout that may accompany feelings of sheer disgust about *both* candidates. The model could also be extended more deeply in time, explicating the genesis of the relationships between prior issue positions, prior party loyalties, and prior votes.

Perhaps the most important consideration to keep in mind is that whereas we have used data from the specific period 1972–76 for estimation of model parameters and assessment of general fit, the model per se should be seen as a shell, or a vessel, designed to apply to a wide range of specific elections. Estimates of specific parameters would naturally vary across types of elections, or for specific elections of the same series over time. To seize an example which is too obvious, for candidate elections which are truly non-partisan in fact as well as intent, the coefficients representing the various points of impact of the party term in the model would by definition fall to zero.

It is this conditional variation from one election to another that becomes of subsequent interest to understand. The present model is thus a satisfying vehicle for providing the kinds of parameter estimates that seem to express the voting calculus well in any special case.

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Reciprocal Effects of Policy Preferences, Party Loyalties and the Vote

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Past studies have offered diverse estimates of the role of policy preferences, party loyalties, candidate personalities and other factors in voting decisions. Most have postulated recursive (that is, one-way) causal relationships among the central variables.

This study specifies a non-recursive simultaneous equation model and estimates its parameters for the 1972 and 1976 elections using CPS data. The estimates differ markedly from those of simple recursive models. Policy preferences appear to have much more influence on voting decisions, and party attachments much less, than was previously thought. Candidate evaluations strongly affect voters' perceptions of closeness to candidates on policy issues. Party identification may be influenced by short-term factors. Differences between 1972 and 1976 reflect the issue-oriented McGovern candidacy.

Simultaneous equation models offer no cure-all; in the absence of accepted theory many specifications are open to controversy. But future research must take account of reciprocal causal paths.

Students of political behavior have long been interested in the extent of "policy voting," that is, the degree to which citizens take account of the public policy stands of candidates when they cast their votes. They have sought to discover the importance of policy orientations in voting, and how they compare with such factors as long-term partisan loyalties and perceptions of the character and personal qualities of the candidates. Answers to such questions are thought to bear not only upon the workings of individual psychology, but also upon theories of democratic politics (in terms of the rationality and cognitive capacities of the citizenry) and upon various theories of the decision process in voting.

Unfortunately, more than 30 years of research have left many issues unresolved. Varying conceptions of the central variables and differing methods of measurement and analysis have led to widely divergent conclusions, even on such apparently straightforward matters as whether policy positions, party loyalties or candidates' personal characteristics were more important in a given election, or whether or not policy concerns increased in importance in the 1960s and 1970s in comparison with the 1950s.[1] It is our purpose to further this research with some new empirical evidence. In doing so we must also complicate matters, suggesting that virtually all past voting studies have erred by ignoring the possibility of reciprocal causal effects among the central variables of the electoral process.

Among the few exceptions is the work of Jackson (1975), whose non-recursive voting model of the 1964 election specified causal interdependence between partisan affiliations of voters and their evaluations of the public policy stands of the parties and candidates. Although we argue below that Jackson's model omits certain crucial reciprocal linkages, it was the first instance of a system of structural equations to consider the roles of partisanship and policy orientations as dependent, as well as independent variables, and represented a major step forward in electoral research. More recent papers by Achen (1976) and Markus (1976) also allow for some reciprocal causal links.

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[1] See Repass (1976), Kessel (1972), Brody and Page (1972).

Within the last several years survey data have become available which allow researchers to design and test sophisticated electoral process models. Yet, while a great many scholars have seized upon these data to improve their operationalizations of central electoral variables, few seem to have recognized all of the implications of the newer constructs (e.g., the "proximity" measures of candidate policy evaluations) for the task of specifying the equation, or system of equations in their statistical models. We will present below a full non-recursive voting model. For data we have turned to the 1972 and 1976 presidential election surveys conducted by the Center for Political Studies (CPS) at the University of Michigan. These are the first election studies to include the measures we believe necessary for constructing an adequate model of the electoral process.

One-Way Causation: Recursive Models of Voting

Most published voting studies to date, from the simplest to the most complicated, have been based upon some form of recursive model of the voting decision. Causation in these models is assumed to operate in one direction only, typically from policy preferences and/or party affiliation and/or candidate evaluations to the vote.[2] We intend to specify and estimate a non-recursive model, in which causation may be reciprocal, simultaneously operating in both directions among several pairs of variables. For comparative purposes, however, we first consider some typical recursive models and their results. We begin with a simple bivariate model which postulates that citizens' policy preferences uniquely determine their voting choice.

We conceptualize the voting choice somewhat differently from usual. Most past researchers have focused on a dichotomous dependent variable—the respondent's prospective or retrospective report of his or her vote for one of the major party candidates. We prefer to analyze the respondent's net comparative evaluation of the opposing candidates, as measured by the arithmetic difference between the scores given to each of the candidates on the CPS "feeling thermometer" scales.[3] The candidate thermometer scores can be argued to represent rather well what utility prospective voters think they would gain or lose from the election of a given candidate. The difference between thermometer scores for Democratic and Republican candidates, then, represents the net gain (or loss) in utility which the citizen would expect to receive as a result of the election of the Republican rather than the Democratic candidate. In this sense, it closely resembles Downs' concept of the "expected party differential" (1957).

Past empirical work has shown that such comparative evaluations of candidates, whether measured by the difference in thermometer scale scores or by a net count of positive and negative open-ended comments about the parties and candidates, are excellent predictors of voting decisions (Brody and Page, 1973; Kelley and Mirer, 1974). In both 1972 and 1976, according to our own analysis, over 95 percent of the voters who scored one candidate higher than the other on the thermometer scale reported voting for that candidate. Ordinary least squares regression of the binary vote measure upon a trichotomization of the net comparative evaluation measure resulted in standardized regression coefficients or betas of .81 for 1976 and .87 in 1972, with corresponding gammas of .95 in both years (see Figure 1).

It can be argued that party loyalties have some direct effects upon the vote in breaking ties when citizens evaluate the opposing candidates equally. We would contend, however, that here partisanship is as likely to be a dependent variable, if anything, and that the apparent additions to explanatory power by party and other variables may merely reflect the imperfection of thermometer scores as measures of the overall utility expected from the election of candidates.

Comparative evaluations, in other words, are so closely related to the vote that they can be substituted for the binary measure in our analysis. Such a substitution has the advantage of providing a continuous variable which can be related to other continuous variables without need for transformations such as logit or probit—which in fact would conflict with our

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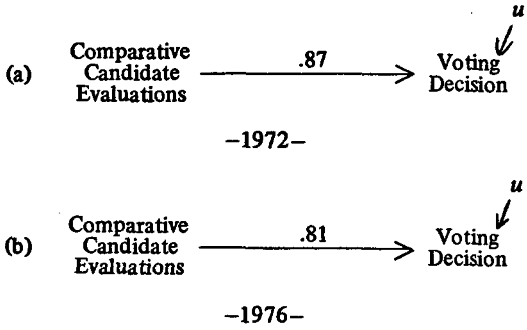
[2] The term "recursive"—literally "running back"—is unfortunate, because it gives exactly the opposite of the correct impression. It does not actually describe relationships between variables, but refers to a property of the corresponding system of equations. It is probably too late to undo the confusion resulting from this usage.

[3] Thermometer scores range between 0 ("very cold or unfavorable") through 50 ("no feeling at all for candidate") to 100 ("very warm or favorable"). In the 1972 survey, when thermometer questions were asked both before and after the election, we used average scores in order to reduce measurement error.


(a)
Comparative Candidate Evaluations
.87
Voting Decision
u
–1972–
(b)
Comparative Candidate Evaluations
.81
Voting Decision
u
–1976–

Source: 1972 and 1976 national election studies, Center for Political Studies, University of Michigan.

Figure 1. Effects of Comparative Candidate Evaluations on the Voting Decision: 1972 and 1976

theoretical specification of an expected utility decision rule—in order to capture more accurately the underlying mathematical function associated with the vote for a particular candidate. It also has the advantage (important for our non-recursive models below) of clearly conceptualizing the attitude immediately proximal to the voting act as one which can conceivably affect, as well as being affected by, other attitudinal variables. Put most simply, we believe that the voting intention is not only a dependent variable, but may be an independent variable as well.

We will now consider several alternative measures of issue orientations, in each case—for the sake of simplicity—dealing with a summary measure of many policy preferences rather than trying to estimate separate effects for different policies. The first, which we will call the "Policy Preference Index," is a weighted linear composite of respondents' scores on a series of closed-ended policy items: the CPS self-rating scales. It resembles the policy index used by Nie, Verba and Petrocik (1976).

A second approach to measuring policy orientations makes use of a simple net count of pro-Republican minus pro-Democratic policy-related comments offered in response to the CPS open-ended questions concerning likes and dislikes for the major parties and candidates. It is similar to the policy variables used by Stokes (1966). This measure (which we have labeled "Net Policy Comments") allows respondents to define their own policy concerns, rather than imposing the same scales of policy alternatives upon everyone: it counts policy orientations as meaningful only when they are salient enough to be mentioned spontaneously by a respondent.[4]

We need a third measure of policy orientation in order to take account of the theoretical literature on spatial modeling which has grown out of the work of Hotelling (1929), Downs (1957), Davis, Hinich and Ordeshook (1970), and others. This literature specifies precisely how policy voting could occur in terms of an individual's decision calculus. It argues that voters take into account their perceptions of the policy proposals of the candidates as well as their own preferences, and that they vote for the candidate whose policy stands are perceived to be closer to their own preferences. Depending upon where the candidates actually stand and where the voters perceive them to be, there might or might not, according to these theories, be a linear relationship between voters' policy preferences and their intended vote. There should, however, always be a direct relationship between comparative policy distances and evaluation of the candidates.

Accordingly, we computed an indicator of "Comparative Policy Distances" between candidates and voters. There are various ways to construct policy distance measures; ours uses the CPS closed-ended, self-rating policy preference scales together with the associated perceptions of candidates' stands to construct a single, relative distance between the respondent and both candidates. That is, absolute distance measures were computed between the voter's preferred position and the perceived positions of the Republican and Democratic candidates on each policy scale. For each voter, distance from the Republican was then subtracted from his or her distance from the Democrat, and the resulting signed, algebraic scores were weighted and summed over all policy scales. The result tells which candidate a voter feels closer to, and by how much.[5]

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[4] We experimented with a number of coding rules governing which comments to count as policy-oriented, ranging from the vague and general to only the most specific. Inclusion of vague comments led to a much larger, and probably inflated, relationship with candidate evaluations. The estimates reported here are all based upon a narrow, specific definition of policy concerns.

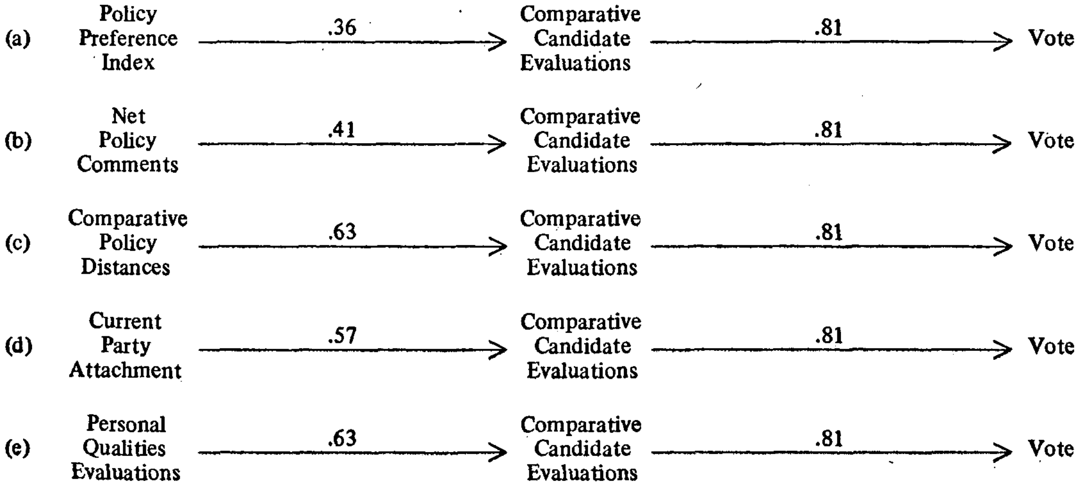
[5] We presume linearity rather than the quadratic loss function specified by Davis et al. (1970) because we see the CPS seven-point scales, with their labeled endpoints but unlabeled interiors, as encouraging respondents to report positions and distances in utility units rather than objective policy units. If they do, the linear relationship with candidate evaluations follows directly from the rationality assumptions of spatial models. Observed relationships are very nearly linear.

Ordinary least squares estimates (standardized) of the effect of issues upon the vote, using each of these three policy measures and the simple bivariate model, are given in Figure 2. Plainly it makes a difference how policy orientations are measured. The first two estimates, based on simple policy preferences and open-ended policy comments, are similar and of moderate size. The latter is slightly larger, suggesting that policy comments may indeed more accurately reflect issues of real concern to voters. But more striking is the fact that the third estimate, using policy distances, is considerably larger than the other two. On the face of it, this seems to support the argument that spatial models do a superior job of specifying the form and process of the voting decision. However, there is an alternative explanation which we will deal with later: namely, that distance or proximity estimates of policy voting may be artificially inflated when the possibility of reciprocal causation is ignored.

In order to round out the bivariate approach to explaining the vote, we included in Figure 2 separate estimates of the effects of two non-policy variables, subjective party attachments and evaluations of the candidates' personal and leadership qualities. Subjective partisanship is measured by the usual CPS seven-point classification of "party identification" based on whether, generally speaking, respondents consider themselves Democrats, Republicans, Independents or whatever. In magnitude, the effect of partisanship ranks between the estimates for "Policy Preferences" and "Policy Comments," on the one hand, and "Comparative Policy Distances," on the other. This result highlights a problem faced by scholars interested in the relative importance of partisanship and policy concerns in the voting process. Obviously, a great deal depends upon how one defines and measures policy orientations at the outset.

We measured reactions to candidates' personalities by counting the net number of pro-Republican minus pro-Democratic comments in response to the CPS open-ended questions, including only those comments focusing on personal or leadership qualities and quite devoid of policy or partisan content. The standardized estimate of the effect of candidates' personal qualities on evaluations is bigger than that for subjective partisanship, policy preferences or policy comments, and equal in size to the bivariate coefficient for our comparative policy distance measure.

The most obvious objection to all these estimates is that they take account of only one independent variable at a time. If policy orientations, partisan commitment and evaluations of candidate character each have some independent influence on the voting decision, and if these variables are even moderately collinear, then the bivariate estimates will be biased.


(a)
Policy Preference Index
.36
Comparative Candidate Evaluations
.81
Vote
(b)
Net Policy Comments
.41
Comparative Candidate Evaluations
.81
Vote
(c)
Comparative Policy Distances
.63
Comparative Candidate Evaluations
.81
Vote
(d)
Current Party Attachment
.57
Comparative Candidate Evaluations
.81
Vote
(e)
Personal Qualities Evaluations
.63
Comparative Candidate Evaluations
.81
Vote

Source: 1976 national election study, Center for Political Studies, University of Michigan.
Note: Symbols for disturbance terms are omitted for the sake of clarity.

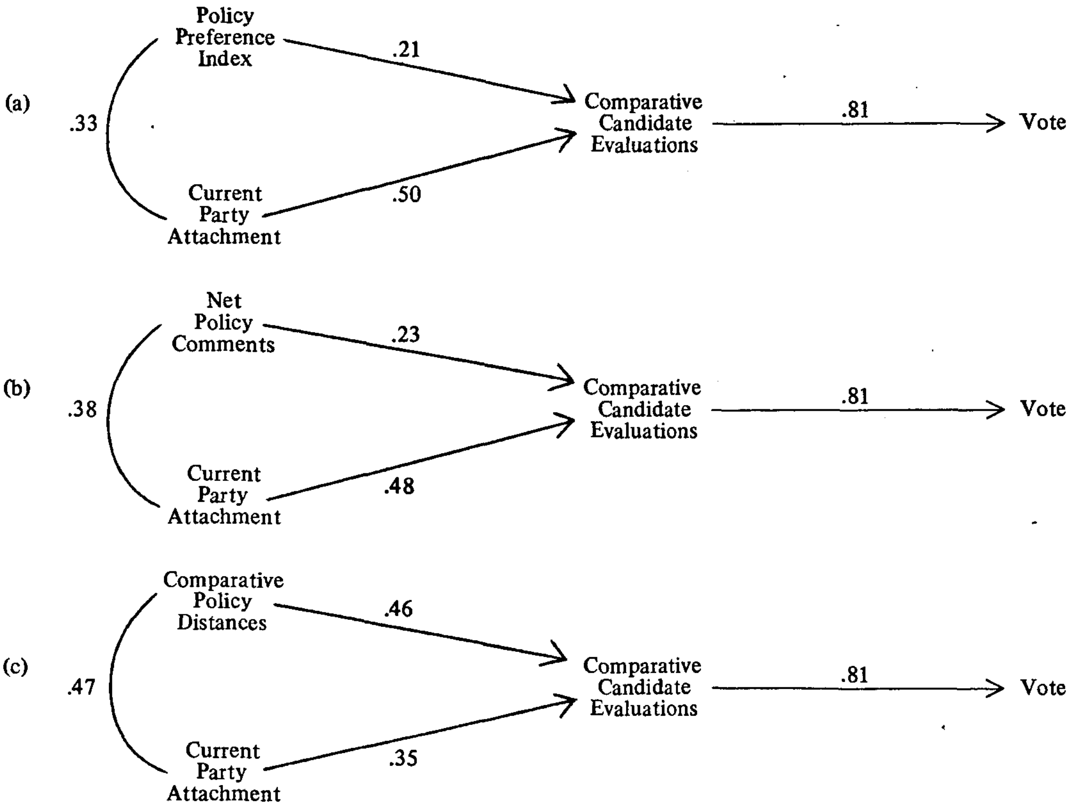
Figure 2. Bivariate Recursive Models of Voting Behavior, 1976

Under the usual circumstances (i.e., positive correlations among the regressors), the bias would be upward, so that bivariate methods are quite likely to overestimate the extent of policy voting—or, for that matter, the extent of party or candidate voting (Johnston, 1972). Some of the work of Boyd (1972), Pomper (1975), Aldrich (1975), Miller et al. (1976) and others is subject to this criticism. Particularly vulnerable is the issue-by-issue "normal vote" technique, since issues may be collinear with each other as well as with other variables.

Of course, most analysts are aware of this problem and incorporate two or more independent variables in their voting models. It is common, for example, to include one or another measure of policy orientations together with partisan loyalties.

Some estimates for such models are shown in Figure 3. As expected, the estimates for both policy orientations and partisanship effects are somewhat lower than those in Figure 2. Party attachments now appear to exert more than twice as much influence upon voting intentions as do either policy preferences or the net policy comments measure. The estimate for policy distances, however, remains higher than that for partisanship when both variables are included, although the disparity is not large.

One can easily add evaluations of the candidates' personal qualities to these models, as in Figure 4. The introduction of an additional explanatory variable results in still further reduction of the estimates for each separate effect. Yet, in relative terms, the initial findings continue to hold: in 1976 voters' evaluations of the candidates' personal attributes appear substantially more important than their partisan attachments in determining the vote choice. Furthermore, both current partisanship and


(a)
Policy Preference Index
.21
.33
Current Party Attachment
.50
Comparative Candidate Evaluations
.81
Vote
(b)
Net Policy Comments
.23
.38
Current Party Attachment
.48
Comparative Candidate Evaluations
.81
Vote
(c)
Comparative Policy Distances
.46
.47
Current Party Attachment
.35
Comparative Candidate Evaluations
.81
Vote

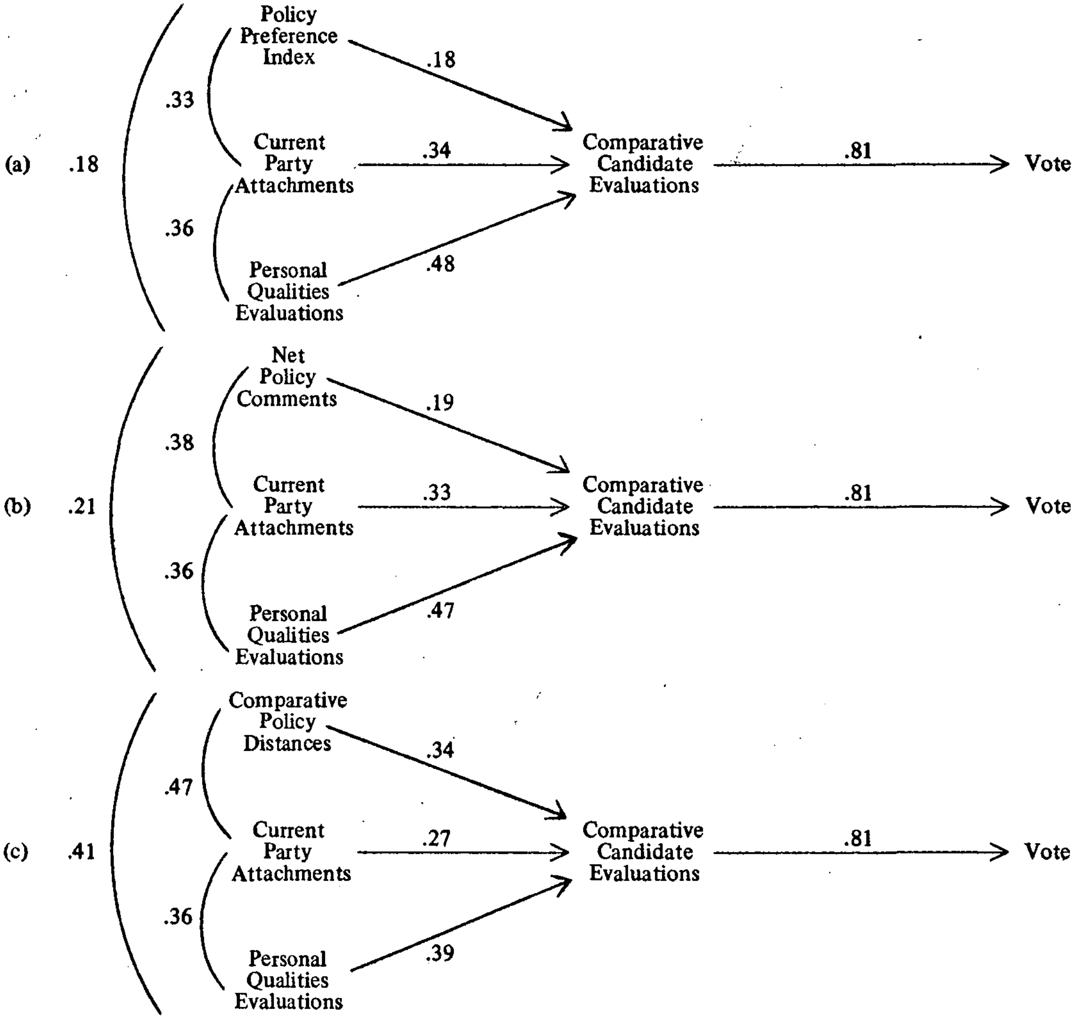
Source: 1976 national election study, Center for Political Studies, University of Michigan.

Note: Symbols for disturbance terms are omitted for the sake of clarity.

Figure 3. Multivariate Recursive Models of Voting Behavior for 1976 with Policy Orientations and Partisanship as Independent Variables

candidates' personal qualities seem to carry far more weight in the voting decision than do policy considerations when the latter are measured by either the index of policy preferences or the net count of policy-oriented comments. If policy concerns are conceptualized as relative distances from the candidates, however, we arrive at a different conclusion. Then, policy considerations seem to be just about as important as personal qualities evaluations in the determination of intended votes. The effect of partisanship is substantially lower.

An important variant of the models in Figure 4 is that formulated by Stokes (1966), which resembles Figure 4b except that a net count of party-related open-ended comments, conceptualized as a "short term force" like issues or candidate personalities, is used in place of long-term party attachments.[6] Making this


(a)
.18
.33
.36
Policy Preference Index
Current Party Attachments
Personal Qualities Evaluations
.18
.34
.48
Comparative Candidate Evaluations
.81
Vote
(b)
.21
.38
.36
Net Policy Comments
Current Party Attachments
Personal Qualities Evaluations
.19
.33
.47
Comparative Candidate Evaluations
.81
Vote
(c)
.41
.47
.36
Comparative Policy Distances
Current Party Attachments
Personal Qualities Evaluations
.34
.27
.39
Comparative Candidate Evaluations
.81
Vote

Source: 1976 national election study, Center for Political Studies, University of Michigan.
Note: Symbols for disturbance terms are omitted for the sake of clarity.

Figure 4. Multivariate Recursive Models of Voting Behavior for 1976 with Policy Orientations, Partisanship, and Personal Qualities Orientations as Independent Variables

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[6] Stokes weights unstandardized regression coefficients by the mean of voters' comments to estimate the net effect upon the electoral outcome of each type

ubstitution decreases the party effect on the ote still further (see Figure 5). If we use this specification, the estimate for policy orientaions (comments) is twice that for partisanship, while that for candidates' personal attributes ncreases to almost twice that for the policy measure and is nearly four times as large as the effect of party attachments.

Two-Way Causation: Non-recursive Models of the Vote

One problem with the preceding analyses is he multiplicity of conceptualizations and specifications. Undeniably, much of the confusion n debates over policy voting stems from the use of different policy measures, different specifications of equations, and different estimation techniques. Researchers simply talk past one another, treating findings as if they were nconsistent when they are not. In principle this problem is easily solved; it is merely necessary to be precise about what is being claimed, rather than talking in global terms.

A more fundamental problem with such analyses, however—a problem confounding most of the existing literature on voting behavior—is an incorrect assumption of one-way causation. The error of this assumption is most obvious in models employing policy proximity or distance measures. Clearly, citizens may tend to vote for the candidate to whom they feel closest on matters of public policy. Yet it seems to us quite possible—in fact likely—that citizens whose initial vote intentions may be formulated on non-policy grounds, can and do convince themselves that the candidates they prefer stand closer to them on the important policy issues. Just such a pattern is suggested by social-psychologists' studies of "projection" or "selective perception"; it also follows theoretically from the rational calculus of citizens operating with less than perfect information. Lacking other evidence, voters might reasonably infer that a candidate who agrees with them on most matters also would agree with them on any new policy matter that comes up. Thus perceived policy distances may be consequences as well as causes of intended votes. There is some empirical evidence, in Berelson et al., *Voting* (1954) and elsewhere, that overall evaluations of candidates do in fact affect perceptions of candidates' policy stands.

By this logic, two-way causation may have biased upward the estimate of the effects of policy distances upon vote intentions in Figures 2c, 3c, and 4c, and it might well bias upward the estimates of policy voting in any recursive model using proximity measures of this kind, even those which "control" for partisanship and other independent variables (Shapiro,

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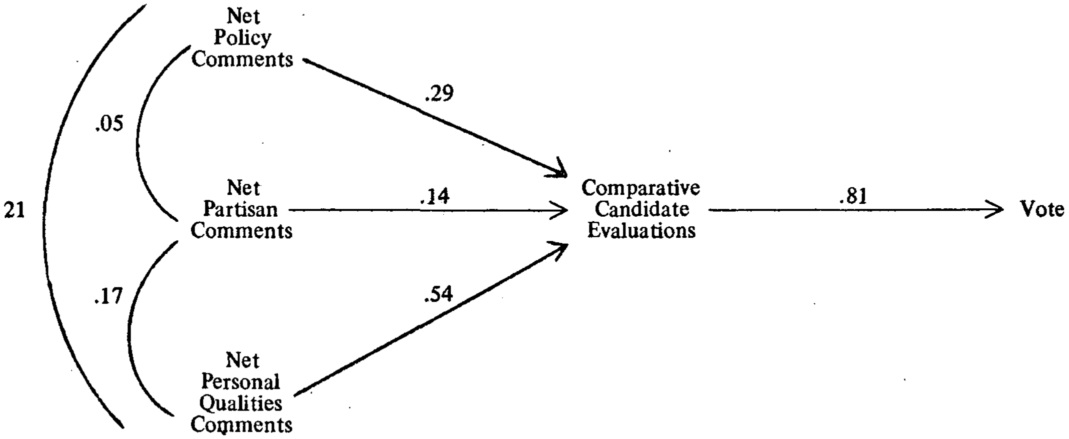
of short-term force. Throughout this article we report he importance of variables only in terms of standardized regression coefficients (*b*'s) and coefficients of determination (R-squares).


Net Policy Comments
.29
.05
21
Net Partisan Comments
.14
Comparative Candidate Evaluations
.81
Vote
.17
.54
Net Personal Qualities Comments

Source: 1976 national election study, Center for Political Studies, University of Michigan.

Note: Symbols for disturbance terms are omitted for the sake of clarity. Policy orientations, partisanship, and personal qualities evaluations measures all based upon net counts of open-ended comments.

Figure 5. Multivariate Recursive Model of Voting Behavior, 1976

1969; Repass, 1971; Aldrich, 1975; Miller et al., 1976).

It must be recognized, however, that much the same problem can occur in voting models which employ policy measures that do not explicitly include perceptions of candidates' stands. For example, recall that policy comments are drawn from responses to the CPS questions concerning party and candidate likes and dislikes. Implicit in each response is a judgment—that is, a perception—about where the parties or candidates stand on policies salient to the voter. Despite the open-ended, voluntary character of these responses, they could still be based upon rationalized and/or incorrect perceptions of the candidate or party positions. In other words, policy comments, too, may be consequences as well as causes of overall candidate evaluations, which again could lead to inflated estimates of the extent of policy voting (Stokes, 1966; Campbell, Converse, Miller and Stokes, 1960; Pomper, 1976; Kagay and Caldeira, 1975).

Moreover, we see no reason to assume that voters' policy preferences themselves are unaffected by general candidate evaluations. When an attractive candidate takes a strong stand on a matter of public policy, might he or she not persuade some supporters—especially those for whom the issue is of comparatively low salience—to bring their opinions into agreement? Or, less grandly, might not some of a candidate's supporters, in lieu of genuine opinions, give facile responses to policy questions corresponding to what they think their preferred candidate stands for? Because candidates are often ambiguous, it can be argued that such persuasion effects are likely to be less important than the effects of projection or selective perception, but we see no reason to exclude altogether the possibility that persuasion occurs.

In short, we are suggesting that all analyses which postulate policy variables to be recursive, uni-directional influences on candidate evaluations or vote choices may be overestimating the extent of policy voting. All the previously cited voting studies (except those by Jackson, Achen, and Markus), and many others not cited, are offenders in this respect. Even the pioneering causal model of Goldberg (1966) is entirely recursive and subject to this criticism, as is the recent work of Nie, Verba and Petrocik (1976).

At the same time, we maintain that the many researchers who do not use policy proximity measures or some other means for taking explicit account of perceived candidate positions are likely to *under*estimate the degree of relationship between policy orientations and intended votes. There is no a priori way to tell for certain which direction of error predominates.

To extend this reasoning a bit further, there is no reason to consider subjective partisanship (i.e., "party identification") to be sacrosanct. It seems quite plausible to us that policy preferences—and relative policy distances from candidates—may be both causes and consequences of party loyalties. Just as opinion leadership by party figures might cause some citizens (even perfectly rational ones) to change their policy preferences, we strongly suspect that some voters' policy preferences—especially those touching on economic, social welfare or racial issues—have some effect on their choice of party in the first place. And, indeed, we would argue that policy distances from particular candidates, who may or may not take positions exactly along the lines of older party cleavages (thus reinforcing or weakening traditional policy profiles of the parties), are likely to affect the strength, if not the direction, of subjective partisanship during a given election campaign. Scholars interested in the relationship between partisanship and policy orientations have typically assumed that causation worked exclusively in one direction between the two, usually from party to issues. They may thereby have inflated their estimates of the effects of party identification (Campbell et al., 1960).[7]

Even Jackson's (1975) pathbreaking non-recursive model postulates that party affects policy preferences but not vice versa. Jackson (1975) and Markus (1976) also rule out any possible influence of candidate evaluations upon policy preferences.

Finally, we must consider the possibility that voters' party loyalties may both affect and be affected by comparative candidate evaluations or intended votes during a particular campaign period. While most researchers have conceptualized partisanship as a sort of blind habit, a relatively long-term exogenous factor affecting vote choice, we expect that to some

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[7] A particularly misleading procedure is the "normal vote" technique. It does not attempt to give unbiased estimates of policy and party effects through regression analysis, but simply assigns the joint covariance of policy and party with the vote in two different extreme ways, so that the reader can choose between "long-term" and "short-term" effects. Except by chance, neither of these magnitudes correctly estimates the extent of either policy preferences or party upon the vote (or of party or policy each upon the other); indeed, the technique does not even identify boundaries around the maximum or minimum possible effects. See Boyd (1972).

(perhaps modest) extent, preferences based on a candidate's character or policy stands will affect the intensity—and at times even the direction—of party affiliations. It seems to us an overinterpretation of the temporal stability of partisan attachments to assume that they are altogether impervious to candidate choices in ordinary elections. Candidates, after all, reflect credit or blame upon the parties that nominate them. Thus the many studies of party identification as a determinant of candidate evaluations or voting choices, from *The American Voter* onward, may have overestimated its effect by ignoring the opposite possibility (Campbell et al., 1960). The Michigan Tradition dies hard.

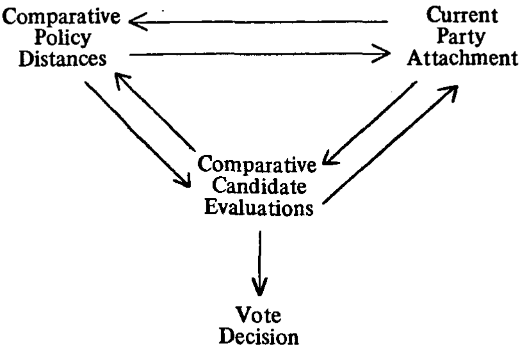
Nor is *The Changing American Voter* immune to this criticism (Nie, Verba and Petrocik, 1976). Comparisons between groups or candidates or over time are also subject to error because the biases in estimates are not necessarily constant from group to group or candidate to candidate or year to year.

We have made the sweeping claim that virtually all studies of policy orientations, partisanship and the vote—certainly including the primitive efforts reported in Figures 2 through 5—are subject to simultaneity bias and are potentially quite misleading. It is fair enough to ask whether we have anything constructive to add to this work of destruction.

The most appropriate way to handle the problem, we would argue, is through the use of non-recursive, simultaneous equation models which explicitly allow for the possibility of causal processes operating in both directions between variables (Johnston, 1972; Theil, 1971; Hanushek and Jackson, 1977; Duncan, 1975).[8] We can begin to apply such techniques to the voting problem by specifying the central set of variables which we believe to be mutually endogenous—that is, which reciprocally affect each other: comparative candidate evaluations, relative policy distance between the voter and candidates, and current subjective partisanship. We will continue to treat measures of reported vote as direct consequences of overall candidate evaluations.

The relationships postulated among these variables are diagrammed in Figure 6. Within the context of this model, we seek to investigate not only the effect of policy orientations upon the vote, but also five other processes of interest: namely, the reverse effect of overall candidate evaluations upon comparative policy distances, and the two-way linkages between current partisanship and policy distances as well as between party attachments and comparative candidate evaluations.

We cannot estimate any of the coefficients in Figure 6, as it stands, because the model is hopelessly underidentified. That is, there are only three empirically observable relationships among the central endogenous variables available to estimate the six causal processes of theoretical interest. Ordinary least squares regression is clearly not appropriate for estimating these causal paths since the required assumptions of independence between the regressors and error terms in each equation cannot be justified. To distinguish between the effects possibly operating in both directions between any two endogenous variables, we must bring additional information into the estimation process. The sort of information needed may be provided by a set of variables which are exogenous to the reciprocal processes specified in Figure 6—variables which can be assumed (on theoretical grounds) to be unaffected by any


Comparative Policy Distances
Current Party Attachment
Comparative Candidate Evaluations
Vote Decision

Source: Compiled by the authors.

Figure 6. Reciprocal Causal Paths in a Non-recursive Voting Model

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[8] Time-series techniques offer an alternative way to sort out causal orderings, but even when time-series or panel data are available, they offer no panacea. In the first place, whether or not measurements are widely separated in time, there is danger in treating lagged variables as causes of current values of the same variables (e.g., party identification): error terms are often autocorrelated as a result of the omission of other factors (e.g., policy preferences) which affect both, and this biases estimates—usually upwards. To capture distinct stages in the development and change of cognitive elements, to ascertain what changes first, measurements might well have to be repeated almost instantaneously. Moreover, quasi-experimental measurements before and after exogenous events are likely to reveal more about the impact of those particular events than about the net mutual influences of different attitudes in the whole electoral process.

endogenous variables, yet which have direct effects on some, but not all, of the endogenous variables (Fisher, 1966).

It is in the search for suitable exogenous variables that difficulties mount, for most of the pertinent social theory is either not very powerful or not universally accepted. The grounds for specifying that a given variable theoretically cannot affect or be affected by another are seldom overwhelming. The situation is worse than usual when one deals with psychological measurements or attitudinal variables, since practically any attitude might conceivably affect any other. There are times when we seem to be studying relationships between mush and slush.

Fortunately, however, it is possible to specify certain exogenous variables for our model with reasonable (and sometimes considerable) confidence, allowing us to identify several of the causal paths. Theory and past empirical research suggest, for example, that the party choices of voters' parents have a direct effect upon their own partisan proclivities, but do not affect their perceived policy distances or overall evaluations of the candidates, except insofar as they act through their own party affiliation. Parents' partisan commitments, therefore, can be specified as operating through uni-directional causal paths upon voters' partisanship only, and their direct effects excluded elsewhere in the model. This will help to identify the effects of voters' partisanship on their policy-oriented evaluations and their vote intentions.[9]

Similarly, we can with some confidence specify that voters' assessments of the character traits and personal qualities of the candidates (as measured by spontaneous responses to open-ended items about candidate likes and dislikes) should have direct effects upon overall comparative candidate evaluations, but should not be substantially affected by overall evaluations in return. In addition, we would argue that character and personality evaluations have no direct links to the voters' policy evaluations of candidates or to their current subjective partisanship, but rather that these effects are transmitted indirectly through the more general overall evaluations of candidates. Thus, the assumption of a recursive relationship from personal qualities assessments to overall candidate evaluations (and the concomitant exclusion of direct effects elsewhere in the model) allows identification of the causal paths from candidate evaluations to both partisanship and comparative policy distances.

Our confidence in this specification depends heavily upon our use only of specific, spontaneous comments about candidates' characteristics, which are far more likely to be causes than effects of candidate evaluations.

It is somewhat more difficult to specify variables which affect voters' comparative policy distances without also affecting the other endogenous variables in the system. We begin, however, by taking advantage of the fact that a number of the voters' background or demographic attributes—factors such as race, sex, age, income and education—affect policy proximities by initially affecting the voters' policy preferences. There is little question that voters' background characteristics are truly exogenous to the electoral process. But the question arises whether they also affect partisanship or general candidate evaluations directly, or whether their effects are transmitted entirely through perceived policy distances. We will assume the latter in our initial model and conduct some tests on the effects of that assumption below.

We have, now, a non-recursive model in which all hypothesized causal paths are identified. The following structural equations are diagrammed in Figure 7.

$$\begin{array}{c}\text{Comparative}\\ \text{Candidate}\\ \text{Evaluations}\end{array} = b_1 \begin{array}{c}\text{Comparative}\\ \text{Policy}\\ \text{Distances}\end{array} + b_2 \begin{array}{c}\text{Current}\\ \text{Party}\\ \text{Attachment}\end{array} = b_3 \begin{array}{c}\text{Personal}\\ \text{Qualities}\\ \text{Evaluations}\end{array} + u. \qquad (1)$$

$$\begin{array}{c}\text{Comparative}\\ \text{Policy}\\ \text{Distances}\end{array} = b_4 \begin{array}{c}\text{Comparative}\\ \text{Candidate}\\ \text{Evaluations}\end{array} + b_5 \begin{array}{c}\text{Current}\\ \text{Party}\\ \text{Attachment}\end{array} + b_6 \text{ Race} + b_7 \text{ Age} + b_8 \text{ Sex} + b_9 \text{ Education} + b_{10} \text{ Income} + u. \qquad (2)$$

---

[9] Since parents' party is measured in the CPS surveys by the respondent's report, it is conceivable that the reported party affiliation of parents is affected by respondents' partisanship through a conscious or unconscious desire to have it be the same (or different), or simply as an aid to faulty memory. Independent studies based on interviews with parents, however, indicate that the bias in the reports is probably not very serious for our purposes (Jennings and Niemi, 1968). This is particularly true since other proximate influences on partisanship reduce the importance of parents' party in our models.

$$\begin{array}{c}\text{Current}\\\text{Party}\\\text{Attachment}\end{array} = b_{11}\begin{array}{c}\text{Comparative}\\\text{Policy}\\\text{Distances}\end{array} + b_{12}\begin{array}{c}\text{Comparative}\\\text{Candidate}\\\text{Evaluations}\end{array} + b_{13}\begin{array}{c}\text{Father's}\\\text{Party}\\\text{Attachment}\end{array} + b_{14}\begin{array}{c}\text{Mother's}\\\text{Party}\\\text{Attachment}\end{array} + u. \quad (3)$$

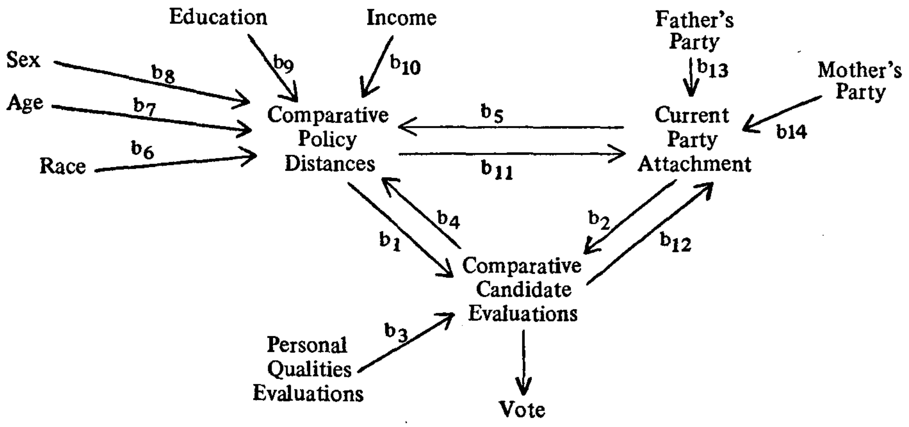
Simply providing for identification of path coefficients does not guarantee adequate specification of the causal model, however, and we believe that additional exogenous variables must be included to overcome biases of a different sort. When important independent variables are omitted from any equation of the model, coefficients for the included variables will still be biased if, for whatever reason, the omitted and included variables are correlated. Several such variables are suggested by past research devoted to the individual equations of our model.

The literature on party identification points to five additional potential influences upon current subjective partisanship: the voter's sense of long-term, traditional involvement with a particular party; degree of consistency in party voting for president; religious preference; region of residence; and general ideological leaning. Some of these factors should carry greatest weight among voters whose party loyalties differ from those of their parents or who were not influenced in any partisan direction by their parents; others mediate parental influence.

For many voters, both the direction and strength of current partisanship may be expected to depend in part upon their degree of involvement with their chosen parties, considered either as reference groups to which loyalty is owed, or as information sources providing interpretations and evaluations of the political environment. We measured this sort of involvement by a net count of positive and negative comments which focus only on parties as traditional referents, wholly excluding both policy and candidate personality connotations: "I've just always been (or voted as) a Democrat," or "I like Nixon just because he's a good Republican."

An even stronger factor affecting current partisanship may be our measure of presidential voting consistency, which combines information about citizens' regularity of voting in presidential elections with data on whether they always supported a particular party for president.

In order to take account of socialization or information sources outside the family for those voters with neither a strong sense of partisan tradition nor a history of regular party voting, we included variables which classify citizens according to their region of residence (10-state South versus all other states) and their religious preferences (Catholics, Jews, Fundamentalist Protestants and those with no religious preference versus all others). Finally, for those voters whose partisanship derives from



Source: Compiled by the authors.

Note: Disturbance symbols have been omitted for the sake of clarity.

Figure 7. Non-recursive Voting Model with Incomplete Structural Equations

their view of the parties as purveyors of more or less coherent social and economic programs, we included a simple trichotomous classification of their ideological leaning based on their self-placement on the CPS closed-ended liberal/conservative scale. Rather than attempt to classify voters on this indicator with excessive (perhaps artificial) precision, we collapsed the seven points into a trichotomy of "pro-left," "pro-right" and neutral positions, combining voters who placed themselves at the mid-point with those who did not place themselves on the scale at all, as ideologically neutral.

To the five background variables exogenous to Comparative Policy Distances, we added a single additional measure—the same ideological classification included in the partisanship equation. Inclusion of so simple a classifying index allows us, without undue worry about reciprocal causation, to estimate the extent to which voters' philosophical orientations not rooted in racial, sexual or socioeconomic differences may affect policy evaluations of the candidates. While attitudes are always suspect when used as exogenous variables, the high salience and stability of voters' self-classification as liberal or conservative to the moderate number of respondents who embrace such a label bolsters this specification. The direction of such a stance is considerably less subject to electoral influence than is the intensity of commitment.

The special nature of the Carter candidacy in 1976 raised the possibility of two additional factors which might have independent effects on comparative candidate evaluations, namely Carter's southern origins and his highly publicized religious outlook. Therefore, we included in the equation for overall candidate evaluations two more exogenous variables. The first is the same indicator of region of residence which appears in the partisanship equation; the second is the dichotomous classification of citizens' religious preferences which also appears in the partisanship equation.

With the addition of these exogenous variables, our model may be described by structural Equations (4) through (6) (diagrammed in Figure 8):

$$\begin{array}{c}\text{Comparative}\\\text{Candidate}\\\text{Evaluations}\end{array} = b_1 \begin{array}{c}\text{Comparative}\\\text{Policy}\\\text{Distances}\end{array} +$$

$$b_2 \begin{array}{c}\text{Current}\\\text{Party}\\\text{Attachment}\end{array} + b_3 \begin{array}{c}\text{Personal}\\\text{Qualities}\\\text{Evaluations}\end{array} + b_4\,\text{Region} +$$

$$b_5\,\text{Religion} + u. \qquad (4)$$

$$\begin{array}{c}\text{Comparative}\\\text{Policy}\\\text{Distances}\end{array} + b_6 \begin{array}{c}\text{Comparative}\\\text{Candidate}\\\text{Evaluations}\end{array} + b_7 \begin{array}{c}\text{Current}\\\text{Party}\\\text{Attachment}\end{array} +$$

$$b_8\,\text{Race} + b_9\,\text{Age} + b_{10}\,\text{Sex} + b_{11}\,\text{Education} +$$

$$b_{12}\,\text{Income} + b_{13}\,\text{Ideology} + u. \qquad (5)$$

$$\begin{array}{c}\text{Current}\\\text{Party}\\\text{Attachment}\end{array} = b_{14} \begin{array}{c}\text{Comparative}\\\text{Policy}\\\text{Distances}\end{array} +$$

$$b_{15} \begin{array}{c}\text{Comparative}\\\text{Candidate}\\\text{Evaluations}\end{array} + b_{16} \begin{array}{c}\text{Father's}\\\text{Party}\\\text{Attachment}\end{array} +$$

$$b_{17} \begin{array}{c}\text{Mother's}\\\text{Party}\\\text{Attachment}\end{array} + b_{18} \begin{array}{c}\text{Traditional}\\\text{Party}\\\text{Involvement}\end{array} +$$

$$b_{19} \begin{array}{c}\text{Partisan}\\\text{Voting}\\\text{History}\end{array} + b_{20}\,\text{Region} + b_{21}\,\text{Religion} +$$

$$b_{22}\,\text{Ideology} + u. \qquad (6)$$

Since each equation of the model includes overidentified paths, we estimated the standardized coefficients for the model using two-stage and three-stage least squares programs.[10] These estimates are also presented in Figure 8.

To us, the most striking aspect of the estimates in Figure 8 is the extent to which they differ from the corresponding coefficients obtained using simple recursive models. Comparative policy distances now appear to be the strongest single factor affecting intended votes, with a standardized, partial (3SLS) regression coefficient of .44. This estimate is greater than that produced by any of the multivariate recursive models examined above; it is roughly comparable in magnitude to the bivariate coefficient for the net policy comments measure (recall Figure 2b above)—although it would be wrong to conclude that recursive models using comment counts necessarily give good estimates of policy voting generally, in all elections.

This major role played by policy orientations in the voting decision is, of course, very much in the spirit of rational-man spatial models. It is noteworthy that our estimation procedure, which takes explicit account of theory by using policy proximity measures, reveals a greater extent of policy voting than do most other methods. At the same time, however, evaluations of the candidates' personal and leadership qualities run a close second in importance, with a standardized coefficient of .38 (about equal to the estimate derived from the recursive model depicted in Figure 4c). Indeed, we cannot be sure that the policy effect was *significantly* greater than that of candidate personality. Spatial models in their narrowest form, postulating exclusively policy-oriented voting, are plainly inadequate. But we would argue that this does not reflect badly on the citizenry. In a presidential system, rational individuals must pay attention to leadership characteristics, if only to ensure that their favored candidate is competent to carry out the promised policy stands.

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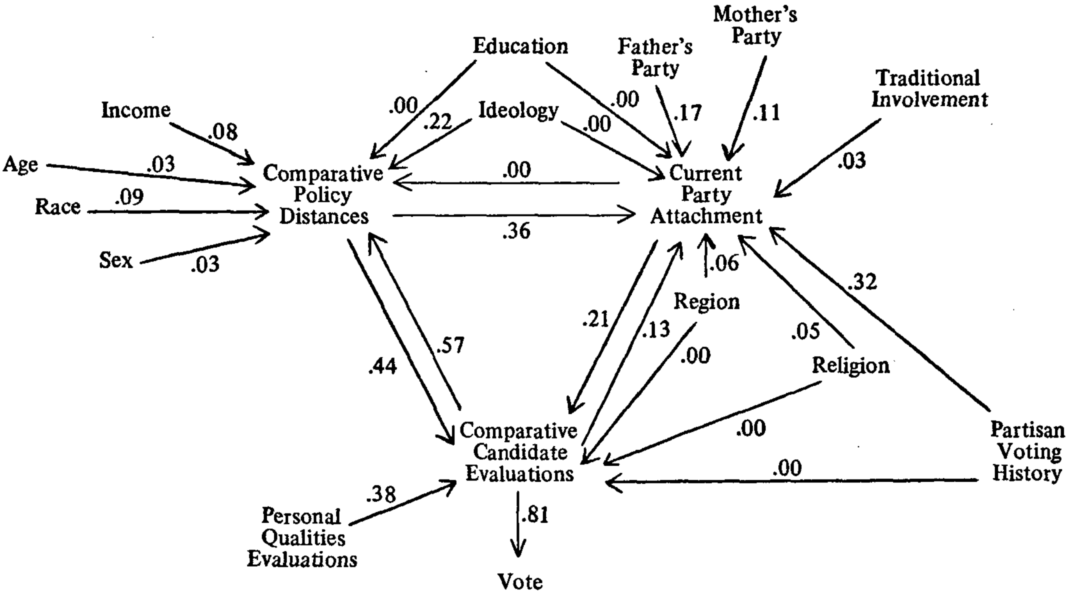
[10] The Statistical Analysis System (SAS) software package was used. The third stage offers improvements in the efficiency of estimates from the previous stage by taking account of the correlations among the equations in the system (Johnston, 1972). In this case, 3SLS estimates differed little from those at the second stage. All variables were standardized before computations were made.

In 1976, citizens' party attachments had much less direct influence upon vote intention than either policy orientations or evaluations of personal qualities. The coefficient of .21 is lower than that for any of the recursive estimates presented above (except that for the attenuated partisan comments measure in Figure 5). Party loyalties, in other words, had some independent effect on the vote—again contrary to narrow notions of voter rationality, but not to a broader conception. (Surely citizens are well advised to view the parties to some extent as governing teams, with records of past performance which bear upon future prospects. What V. O. Key called the "standing decision" to support one or another party—subject, of course, to modification in the light of experience—is an efficient and generally reliable aid in voting.) Yet our estimate indicates that partisanship was by no means a straitjacket. It was clearly *not the most important factor* affecting the vote, as has been alleged of the 1950s and early 1960s.

Despite the special features of Carter's campaign, in 1976 the estimated coefficients for both region and religion did not differ significantly from zero. We suspect that this result follows from the fact that evaluations of personal qualities and current partisanship already take account of such effects. With these variables included in the candidate evaluations equation, the region and religion variables are superfluous.

Two important findings, which can emerge only from a non-recursive model like ours, concern the reciprocal effects of both intended



Source: 1976 national election study, Center for Political Studies, University of Michigan.

Note: Disturbance symbols and inter-correlations among exogenous variables have been omitted for the sake of clarity.

Figure 8. Full Non-recursive Voting Model with Overidentified Structural Equations, 1976

votes and policy distances upon current partisanship. When all of the customary influences on the formation and maintenance of party attachments are allowed for, intended votes still show a modest independent effect (.13) on voters subjective partisanship. In 1976, then, partisanship was not an exogenous influence on the vote, impervious to other electoral stimuli. Instead citizens apparently alter their party loyalties when the parties nominate especially desirable or undesirable candidates, just as sensible voters would be expected to do. By ignoring this effect of overall evaluations of the presidential candidates upon partisanship, many scholars have overestimated the effects of party affiliation upon voting decisions.

What may be more surprising to some is the much stronger effect (.36) of comparative policy distances from the candidates upon party attachments. Conceptually, it seems logical enough that voters who see their policy interests served much better by one candidate than another might be inclined to consider themselves supporters of that candidate's party. The question arises, however, why such a judgment on policy grounds would not be translated into an overall positive evaluation of (i.e., an intention to vote for) that candidate, and only then go on to influence partisanship. The answer seems to involve the fact that policy judgments form only one of many independent dimensions of candidate evaluations; preference for one candidate on policy grounds does not guarantee a favorable evaluation overall. In fact, in 1976, over 15 percent of the survey respondents fell at the neutral or indifference point on our measure of comparative candidate evaluations. While 65 percent were classified as being closer to Carter on policy matters (35 percent closer to Ford), only 33 percent preferred Carter's personal and leadership qualities and 40 percent preferred Ford's. Thus even when comparative policy distances did not automatically lead to vote intentions, they appear to have influenced voters' feelings of attachment to the political parties.

For a given election, the overall magnitude of either of these endogenous effects on partisanship would depend, of course, upon citizens' partisan attitudes before the start of the campaign and upon their various evaluations of the nominees. The effects are not overwhelmingly large; we would not argue from these findings that major shifts of pre-existing party allegiances result from a campaign like that of 1976. The several variables which we specified as exogenous to current party attachment have significant (and in some cases, sizeable) coefficients, clearly indicating the presence of powerful stabilizing influences. The effects of parental socialization and of consistency in party vote for president, for example, no doubt serve to anchor some voters against the ebb and flow of campaign developments. Yet change in party attachment, whether through the weakening or strengthening of former ties, or by the creation of a sense of partisanship among new voters, clearly followed from evaluations of campaign stimuli. This finding, we hope, will help lay to rest the notion that partisanship can be treated as an unmoved mover in the analysis of voting behavior.

Perhaps the most theoretically important of all our estimates is the strong effect (.57) of overall candidate evaluations upon perceived policy distances. The existence of such an effect squarely controverts the assumptions of spatial models, which specify that voting decisions are the result of calculations of policy proximities or distances from the candidates, and that the policy preferences and perceptions of candidates' stands used in these calculations are independently arrived at. The evidence is clear that influence runs both ways: voters' evaluations of Carter's and Ford's policy stands were strongly conditioned by their overall judgments about the candidates. Those who generally favored Carter, let us say, tended to locate themselves and Carter close together on the policy scales; those who disfavored a candidate tended to place themselves and that candidate farther apart. This finding goes against the fixed-preferences and perfect-information assumptions widespread in economists' views of politics (and, for that matter, economics as well). It calls for considerable rethinking of some notions of electoral democracy.

At the same time, it would be quite erroneous to conclude that we have found voters to be irrational or deficient with respect to some democratic ideal. In the first place, the influence of candidate evaluations upon perceived policy distances from candidates encompasses a complex bundle of processes (projection, persuasion, rationalization), in which either voters' policy preferences or their perceptions of candidate stands are affected by their evaluations of the candidates. Some of these processes are useful or even essential to the workings of democracy; we cannot at present sort out which are operating. (To distinguish the effects on perceptions from those on preferences would require a four-equation model with two complicated nonlinear equations.) In the second place, most if not all of these processes are best understood as reactions by rational citizens to the problems of obtaining political information. To the extent that some reactions interfere with electoral democracy, much of the blame must fall upon the political environment and the kinds of information made available (or lacking), rather than upon the voters themselves.

It is quite possible—and compatible with our findings—that some voters who have no information about the candidates' policy stands, but who have developed a preference for one candidate (based on party ties or personal attributes) project their own preferred policy positions onto their favorite candidate, especially when pestered to answer survey questions about where candidates stand. In addition, there are no doubt voters for whom policy considerations are of sufficiently low salience that they blithely adopt as their own the policy prescriptions of whichever candidate they have come to prefer for non-policy reasons. Yet persuasion (i.e., a candidate's influence upon policy preferences) can take a much less casual form—a form equally compatible with our findings—when citizens listen to information and argument about policy and are genuinely convinced by a candidate they trust. In a complicated world of imperfect information, rational citizens must be open to persuasion.

Moreover, the existence of strong effects of policy orientations upon candidate evaluations suggests that few voters, if any, rely exclusively upon simple rationalization in arriving at their final evaluations of the presidential contenders' policies. It is much more likely that a given voter will have had at least some realistic notion of his or her comparative distances from the candidates which conditioned more general evaluations in the first place. Lacking perfect information, such voters might then combine what data they have on policy distances with party cues and judgments about the candidates' personal attributes into a preliminary comparative evaluation of the nominees, which they then may use to infer their ultimate net distance from the candidates over a wider range of policy areas. In this light, what at first seemed to be rationalization may in fact be rationality.

Again, we would emphasize that if voters make foolish and easily avoidable errors about candidates' policy stands (and no evidence yet establishes this to be so), this probably results as much from the obscurity and ambiguity of what candidates say as from limitations of citizens' cognitive processes (Page, 1978, Ch. 6).

All the findings we have discussed so far pertain to the 1976 presidential election. Both as a check on our methods and for its own substantive interest, we present in Figure 9 comparable estimates for 1972. All measures were constructed in precisely the same way as for 1976, and the series of alternative specifications used for testing purposes were identical.

Comparison of Figures 8 and 9 reveals many similarities in overall structure, especially in the effects of exogenous variables. This increases our confidence in the model and the estimation procedure. At the same time, the comparison uncovers some sharp differences, which are readily interpretable in terms of differences in the electoral environment. Plainly the Carter-Ford contest and the Nixon-McGovern campaign differed in ways which aroused dissimilar reactions among the voters.

As in 1976, we find in 1972 clear evidence of reciprocal causation between policy orientations and overall candidate evaluations. Many citizens voted on policy grounds, but many also were influenced in their assessment of policy distances by their overall evaluations of the candidates. As in 1976, the effect of candidate evaluations upon perceived policy distances (.81) appears slightly stronger than the effect in the opposite direction (.76), but this difference was much smaller in 1972 than in 1976 and is not statistically significant. No doubt the projection or rationalization of issue stands was relatively more difficult in the "clear choice" setting of 1972.

What stands out most strongly is the much greater magnitude of both roles of policy orientations (as both independent and dependent variables) in 1972. The Nixon-McGovern election was truly an issue-oriented affair; no other factor approached the importance of public policy orientations in affecting voting decisions. Indeed, this feature of the election was so marked that it can be discerned no matter how crudely the data are analyzed, as when we replicated for 1972 various simple recursive models. Yet, at the same time, the clarity of the choice on policy grounds evidently did not prevent a very substantial amount of alteration in perceptions of candidate stands and/or policy preferences in response to feelings about the candidates.[11]

The impact upon votes of citizens' evaluations of personal qualities was surprisingly low (.15) in 1972, suggesting that explanations of the vote which emphasize McGovern's allegedly poor judgment and personal instability as important direct factors are probably somewhat wide of the mark. According to our estimates, policy distances had an effect over five times as great as did reactions to the candidates' personal attributes. Still, this finding is not inconsistent with the possibility that the outcome of the election (as opposed to the total variance in voting decisions) was heavily influenced by judgments about the candidates' personal and leadership qualities. Calculations of Stokes-type net effects, which take account of the strongly anti-McGovern drift of this factor, indicate that it was quite important (Popkin et al., 1976).

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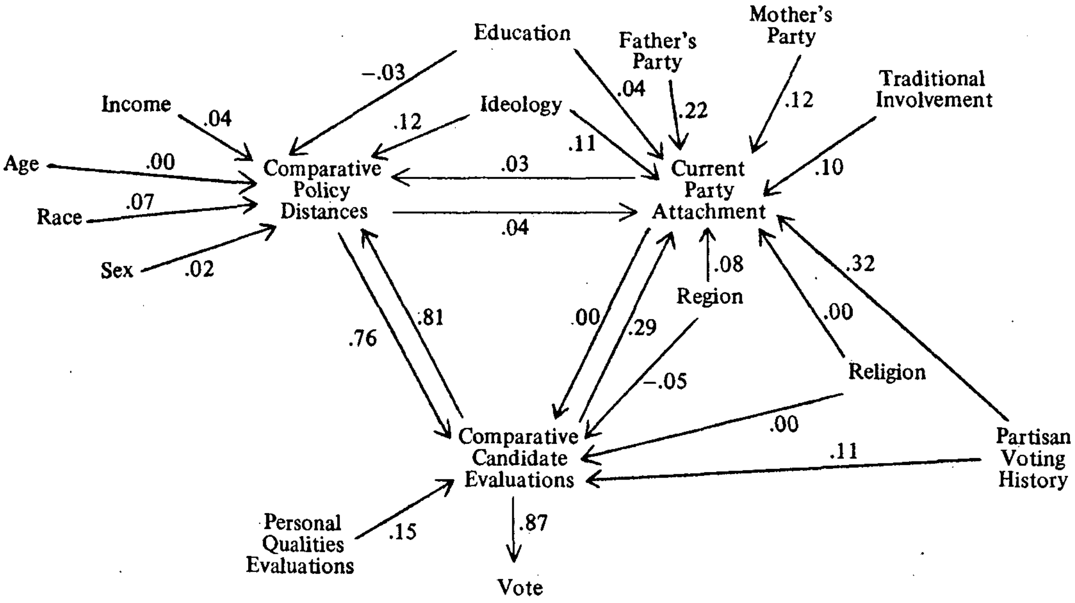
[11] In point of fact, the clarity of the choice in 1972, like that in 1964, actually left much to be desired. Nixon's policy utterances were vague throughout, and McGovern gradually retreated into ambiguity after taking unusually specific stands on the defense budget, income redistribution, and the like (Page, 1978).

Among the most noteworthy aspects of Figure 9 is the nearly total absence of any effect of party attachments upon the other two endogenous variables. Our estimates reveal that in 1972–quite unlike 1976–party loyalties played no part in the formation either of voting decisions or perceptions of closeness to the candidates on policy matters. McGovern dramatically dissociated himself from the Democratic party's mainstream; and, by the same token, the central core of the Democratic party abandoned McGovern to his own devices. Party *per se* had no independent effect at all on the vote. Furthermore, the absence of a significant effect of policy distances upon partisanship, together with the very strong impact of policy distances upon intended vote, indicates that the policy choices around which voting decisions revolved (the Vietnam War, urban unrest, campus disturbances, alternative life styles) cut across the grain of older party cleavages.

There was, on the other hand, apparently some effect of intended votes upon partisanship. That is, although party loyalties did not affect votes, intended votes did affect partisanship–presumably because some Democratic defectors to Nixon felt their Democratic affiliations to be weakened. There are, however, some doubts about this finding. In spite of the moderate size of this coefficient (.29), the conventional significance test (which is asymptotically valid for two- and three-stage least squares estimation) indicates that it is not significantly different from zero at the .05 confidence level. Examination of the intermediate two-stage calculations reveals a high degree of collinearity between the "decontaminated"



Source: 1972 national election study, Center for Political Studies, University of Michigan.

Note: Disturbance symbols and inter-correlations among exogenous variables have been omitted for the sake of clarity.

Figure 9. Full Non-recursive Voting Model with Overidentified Structural Equations, 1972

versions of the policy distance and candidate evaluations variables. This results in high standard errors of estimates for the respective coefficients (i.e., lowers their precision), which substantially raises the difficulty of disentanglng the independent effects for these two variables. Since we presently lack any satisfactory way to deal with multicollinearity in the second stage of two-stage least squares, it is necessary to reserve judgment on the significance or insignificance of the 1972 impact of ntended vote upon partisanship.

Among the exogenous factors in 1972, there are also some differences from 1976 related to the nature of the candidate pairings in those two years. For the most part, however, the exogenous variables—especially those which represent stable characteristics of respondents—exerted roughly comparable amounts of nfluence in both elections. In general, the overall structural similarity between the estimated models for 1972 and 1976 is an encouraging sign that the estimates are correct and not subject to vagaries of the estimation technique or arbitrary variations with independent data sets.

Thus the variation in the effects of ideoogical orientations is easily explained in terms of the differences in the kinds of policies at ssue during the two campaigns. In 1976, policy debates tended to line up much more with older, New Deal notions of liberalism and conservatism than was the case in 1972. Our measure of ideological leaning (which relates most closely to voters' attitudes on New Deal type issues) therefore had a greater effect on policy distances in 1976 than in 1972. In addition, whereas in 1972 voters' ideological orientations affected both policy distances and party attachments directly, in 1976 the effects of ideology were passed entirely through policy distances, affecting partisanship indirectly via the path from policy distances to partisanship.

Changes in the pattern of effects for partisan voting consistency are of a similar nature. In 1972, party attachments had no discernible effect on comparative candidate evaluations. Yet the more consistently citizens had voted for the same party for president, the more ikely they were to prefer that party's candidate to the opposition in 1972. Presumably it took a rather special kind of partisan attachment ooted in a firm history of voting support to make Democrats (independently of other facors) prefer McGovern to Nixon. In 1976, however, we find that the direct path from voting history to comparative evaluations no onger functioned. Again, controlling for other factors, voting history directly affected only current party attachments, with candidate evaluations being affected only indirectly.

Finally, it seems to us that the differences in the effect of personal qualities evaluations between the elections is largely due to the great differences in the intensity of the effects of policy issues in the two campaigns. Policy distances were so overwhelmingly important in 1972 that judgments about personal attributes were left with relatively little independent explanatory power.

Conclusions

The clearest implications of our findings concern errors inherent in recursive voting models. In the first place, researchers who rely on single-equation techniques simply fail to reproduce faithfully the underlying complexity of the electoral decision process. Beyond this, even if multiple equations are specified in elaborate hierarchical recursive models, the estimated coefficients are still subject to simultaneity bias.

This defect is most damaging when recursive models are used with variables (such as the policy distance measures) which are especially likely to have been influenced by the very factors of which they are presumed causes. Our results suggest that the use of such measures should be restricted to non-recursive simultaneous equation models and that only those estimation techniques appropriate for such models should be applied. Yet practically all studies of issue voting, party identification and the like have specified uni-directional causal relationships without having theoretical justification for doing so. Our findings indicate that the degree of error introduced by mis-specification and simultaneity bias in such models can be quite substantial.

Our most important empirical discovery involves the reciprocal causal paths between policy orientations and overall candidate evaluations. In both 1972 and 1976 there was a high degree of policy-oriented voting—considerably more than is revealed by some less powerful analytical techniques. The identification in both years of a substantial effect from intended votes to policy distances, as well, confirms what many theorists have suspected for some time: policy-based evaluations of presidential candidates are endogenous to the electoral process.

We have argued that the effect of intended votes upon policy distances is consistent with individual rationality, since it may be perfectly rational to be persuaded by a favored candidate's policy stands, or even to infer a candidate's positions on policies (and, hence, one's degree of proximity to them) from other characteristics. We have pointed out, however, that this is inconsistent with simple spatial models of voting behavior, in which preferences are assumed to be fixed, and perceptions of candidate stands are assumed to vary only randomly (if at all) among the electorate.

In addition, the estimates from our two data sets imply that the effect of partisanship on the vote varies considerably across elections, depending largely upon the nature of the candidate pairings and the extent to which current policy issues conflict or coincide with established party cleavages. When the policy debates of a campaign are such that the parties are seen to have relatively distinct and internally coherent positions, and when the presidential nominees are perceived as being reasonably typical representatives of their respective parties' interests and stands (as was largely the case in 1976), then voters' current party attachments may both affect and be affected by policy orientations and overall candidate evaluations. When these conditions are not met (and surely 1972 was a quintessential case where they were not), partisanship is isolated from the electoral process.

Further, when party loyalties do enter in, they do not function purely as fixed determinants of the vote; those loyalties can themselves be affected by attitudes toward the current candidates. Even short of major realignments, party affiliations are effects as well as causes in the electoral process.

We have contended that non-recursive simultaneous equation models are necessary in order to eliminate substantial, systematic baises from estimates based on cross-sectional data. At the same time, we must concede that the modeling procedures and estimating techniques we advocate are by no means free from specification problems. One of the chief difficulties is locating variables in existing data which are genuinely exogenous—that is, which are truly free from reciprocal influence by their target endogenous variables, and which can be excluded on persuasive theoretical grounds from having direct effects on certain other endogenous variables. In the search for plausible and identifiable specifications, researchers must be quite careful to state their theoretical justifications, and also to make clear, by the use of sensitivity testing, just what the consequences of alternative specifications would be.

Simultaneous equation analyses are, to an important extent, dependent for their success upon the decisions of those who design survey instruments and collect the data. If important variables are not measured or are measured poorly, one cannot place much confidence in empirical findings. By "important variables," we mean not only those of major theoretical and substantive interest, but also potential exogenous variables, some of which may be of little interest in themselves but which have the theoretical properties necessary to assist in estimation.

Lacking tailor-made measures, we have tried to exercise great caution in the specification of exogenous variables—choosing wherever possible items on which voters had fixed characteristics, or had reached established values prior to the campaign under study. Where this was not possible, e.g., in the cases of evaluations of candidates' personal qualities or of voters' ideological leanings, we sought constructs which approached the theoretical ideal as closely as possible. Our measure of personal qualities evaluations, for example, was operationalized so as to minimize the possibility that individual voters' scores would be affected by other factors, while maximizing the probability that they would reflect the true extent to which personal qualities evaluations formed the basis of the voter's electoral decision. We doubt that this construct perfectly satisfies all of the theoretical requirements of an ideal exogenous variable. We believe, however, that it is a defensible specification, and that any biases introduced by this type of measure will be small indeed when compared with those resulting from single equation or recursive estimation. The estimates of effects from candidate evaluations to policy distances and partisanship, which depend heavily upon this specification, may be slightly inflated, but we are satisfied that they are very nearly correct.[12]

Future presidential elections will no doubt be accompanied by voter surveys of varying purposes and designs. The opportunities for further developments in non-recursive modeling remain open on a good many fronts. The refinement of existing exogenous measures, the use of panel studies and the incorporation of quasi-experimental designs in future surveys all offer possibilities for improvements in the analytical techniques advocated here. Even limited success in this direction will add far more to our understanding of voting behavior than will repetition of error-ridden, mis-specified models.

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[12] Extensive sensitivity testing was conducted using a series of alternative specifications for each of the three equations. For example, personal qualities evaluations were tested for direct effects in both the partisanship and the policy distances equations, with negative findings. Similar testing showed that the potentially controversial specification of some demographic variables as affecting perceived policy distances but not (directly) partisanship, has surprisingly little effect upon the estimates among endogenous variables. In fact, so long as parental partisanship was included in the equation, none of the demographic variables was found to have significant effects upon current party affiliations. In the same vein, when our predictors of partisanship were inserted in the equations predicting overall candidate evaluations and policy distances, only those specified in our final models showed significant direct effects.

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Political Participation and Government Responsiveness: The Behavior of California Superior Courts

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Students of democratic politics have long been concerned with the role of political participation in linking government and the people it serves. Whereas participation is generally defined in terms of voting, this article defines participation as the communication of citizen preferences to public officeholders. We show that aggregate sentencing decisions of California superior courts changed to reflect more closely prevailing public opinion after a large percentage of the populace expressed their preferences on a marijuana issue. The fact that members of California superior courts are seemingly immune from any effective electoral sanction serves both to underline the importance of this form of participation to a responsive system of government and to caution against conceiving of the participation-responsiveness relationship only in terms of punitive electoral devices.

In their major work on political participation in the United States, Verba and Nie state (1972, p. 300), "Responsiveness is what democracy is supposed to be about and, more specifically, is what participation is supposed to increase." Although this theme is prominent among students of American politics, a satisfactory answer to the question, "Does participation make a difference?" is not apparent, in part because researchers have tended to equate participation with voting. It is natural that scholars should focus their attention on the most common mode of participation, but concentration on voting also seems to reflect an underlying assumption that the predominant if not single purpose of participation is to exert pressure on public officials. Mass voting presumably sustains politicians' fear of being tossed out of office, which in turn ensures that government will remain sensitive to the citizens it serves (cf. Schumpeter, 1942). Widespread acceptance of participation as coercion is patent: most research on government responsiveness has focused on legislative bodies, whose members are popularly elected to short terms of office.

Verba and Nie (1972, p. 47 ff.) remind us that participation can serve a second and distinct purpose: to communicate information about citizen preferences. Here efforts to influence government take the form of increasing officials' awareness of the public's wishes. Whereas electoral coercion involves applying diffuse pressure without specifying *how* leaders should respond, citizen communication of preferences provides relatively precise guidelines within which officials are to make policy decisions.

As a research matter, considering participation as a communication activity has two positive consequences. First, it underscores the often-overlooked point that responsiveness is best construed as a *configurative* phenomenon, involving officials and the mass public simultaneously. To conclude that government is unresponsive when the public is apathetic or noncommunicative may be ill-conceived, or at best trivial. Even government leaders dedicated to carrying out the public will would be hard put to do so if that will were unknown. Second, interest is extended to non-elected officials. Study of the participation-responsiveness nexus can include the large proportion of government officials (many bureaucrats and judges, for example) who are isolated from any real electoral sanction but who play important, often

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decisive, roles in the policy-making process.

Only recently have researchers begun to look at the relationship between this second function of participation and government responsiveness (Verba and Nie, 1972; Hansen, 1975, 1978; Stone, 1976; Karps, 1978). They have measured, typically, the congruence of mass public and elite *attitudes,* from which *behavior* can only be *inferred.* The use of data collected at a single point in time, moreover, precludes separating agreement resulting from simple elite-mass sharing of policy attitudes and that due to the actual response of officeholders. In short, the causal link that the authors seek to identify is remote at best.[1]

The data reported in this article provide a somewhat unique opportunity to consider more directly than earlier work whether participation, henceforth defined as the communication of policy preferences, encourages public officials to respond. As a corollary to this basic question, we shall speculate about the centrality of electoral coercion to the relationship. To find that the communication of preferences inspires government action, even in the absence of direct electoral challenge of officials, would underscore the connection between the availability of information about citizen preferences on the one hand and the responsive quality of our representative institutions on the other.

Data, Method, and Level of Analysis

When participation is viewed as an instrumental act through which citizens try to influence government, a systemic or "macro" perspective is of considerable value. This perspective not only allows the researcher to determine whether aggregate communicated preferences affect the collective decisions of a government decision-making body, but it also accords with Pitkin's explication of responsiveness (1967; see also Prewitt and Eulau, 1969) as an emergent property of a complex system of interactions among citizens and government officials.

In keeping with this perspective, we have chosen the California superior court as our unit of analysis (Cook, 1967, pp. 65–66). The superior court can be regarded as consisting not only of the judges who sit on the bench,[2] but also of the police, parole officers, prosecutors, and defense attorneys whose interactions shape its final decision. Because these lower courts are legally defined as policy-making bodies having general jurisdiction coterminous with the state's counties, it is possible to ask whether the decisions of the courts are related to variations in public preferences across the counties, and whether this relationship, in turn, is affected by citizen communication of policy preferences.

These questions require data on both the public's preferences and judicial decisions within a specific policy area. We chose as a measure of opinion aggregate voting returns on an initiative ballot proposition proposed during the 1972 general election to remove criminal penalties for the personal use of or preparation for personal use of marijuana. As presented to the voters, the initiative reads:

> [This marijuana proposition] removes state penalties for personal use. Proposes a statute which would provide that no person eighteen years or older shall be punished criminally or denied any right or privilege because of his planting, cultivating, harvesting, drying, processing, otherwise preparing, transporting, possessing or using marijuana. Does not repeal existing, or limit future, legislation prohibiting persons under the influence of marijuana from engaging in conduct that endangers others (State of California, 1972, p. 31).

For each county we use the percent support for the marijuana issue.[3] The higher the percentage, the more liberal is a county toward the personal use of marijuana.

In addition to providing a valid measure of public preferences, the initiative vote serves our research objective in two ways. First, it *is* an initiative. Members of the public both brought the issue and chose to express themselves on it. Over 90 percent of those voting in the general election registered their opinions on the marijuana question. Second, since the vote itself represents a participatory act, we can ask whether participation leads to government response by comparing the levels of policy agreement before and after the communication of preferences.

We therefore computed a measure of judicial policy for each of three years—1971, 1972, and 1973. The policy measure is the mean sentencing severity score of all decisions involving possession of marijuana convictions. It is based on a scale first proposed by the Administrative Office of the United States Courts and subsequently employed by Cook (1973), Tiffany et al. (1975), and Gibson (1976). The general categories of the scale are: (1) suspended sentence, (2) unsupervised probation, (3) fine, (4) supervised probation, and (5) imprisonment. Since we are looking at only marijuana convictions, we adjusted the original scale to discriminate among sentences more finely.[4]

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[1] A notable exception to both criticisms is Stone (1976).

[2] At the time of this study, the number of judges on the bench ranged from 161 in Los Angeles County to one per court in the least populated counties.

[3] Support for the referendum varied from over 50 percent in the Bay Area to less than 20 percent in some southern counties.

Sentencing scores were computed for each superior court by year. For purposes of analysis, we have excluded for any given year courts with less than five convictions in that year.[5]

The marijuana decisions, from which the judicial policy measure is constructed, are ideal to study the relationship between political participation and government response. For one thing, the state legislature has given broad sentencing discretion to California superior courts. During the period under examination, judges had authority in marijuana cases to impose prison terms of one to ten years, to order jail sentences, to fine offenders, or to suspend any sentence with or without probation. For another, both the sentencing decisions and public opinion deal specifically and only with the simple possession and use of marijuana. There should be no doubt that we are measuring a direct and well-defined policy linkage between the public and one set of its officials.

Participation and Response

To explore whether political participation has an effect on government behavior, we employ, initially, the simple regression model

$$\begin{matrix}\text{Sentencing}\\ \text{Severity}\end{matrix} = \alpha - \beta \begin{bmatrix}\text{Policy}\\ \text{Preference}\end{bmatrix} + \epsilon \tag{1}$$

for each of three years. Our hypothesis is that the estimated regression coefficient of the 1973 equation (disregarding the sign) will be larger than those of the two earlier years, given the communication of policy preferences.[6]

The least-squares estimates reported in Table 1 confirm the hypothesis. While public preferences are related to sentencing in the expected direction in 1973, the coefficients for the two earlier years are not significantly different from zero, as determined by the *t*-ratios.[7] A compari-

Table 1. The Relationship of Public Preference and Sentencing Severity, Marijuana Cases, 1971–1973

Year	Intercept (α)	Public Preference* (β)	R	$\bar{R}^2$
1971 (N = 47)	133.3	–.58 (1.9)	.28	.08
1972 (N = 47)	120.1	–.32 (1.2)	.17	.03
1973 (N = 45)	140.8	–.84 (2.4)	.44	.19

Source: State of California, *Statement of the Vote* (1972); California Bureau of Criminal Statistics.

*Figures in parentheses are ratios of the regression coefficients (disregarding signs) to their standard errors (*t*-ratios). Values between 2.0 and 2.3 indicate significance at the .05 level; values greater than 2.3 indicate significance at the .01 level.

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[4] Our adapted scale, for example, enables us to treat incarceration of any length as a more severe sentence than sentences not imposing incarceration but requiring long periods of supervised probation. The scale ranges from 0 to 150. Although not interval in the strictest sense, it is used as an interval measure here.

In addition to calculating the sentence severity of those convicted of possession of marijuana, we computed the sentence severity of those charged with possession and convicted of the original charge or a lesser offense. When the latter measure was used as the dependent variable, the relationships were virtually identical to those we report. We employ the former measure because we lose fewer cases with it. We also used as a dependent variable the percentage of dismissals in possession cases. We did not find this measure to relate to public opinion in any of the three years.

[5] All the eliminated courts had only one judge.

[6] An underlying assumption of the regression model is that officials will interpret a county's initiative vote relative to the returns of the other counties. This makes sense, since some sort of referent is needed to give a county initiative vote meaning.

[7] Given the theoretical plausibility of a nonlinear relationship between opinion and sentencing policy, we were careful to examine for such a relationship for each of the three years. None was identified. Given the small N's, we also confirmed the stability of the

son of slopes test, moreover, shows that the 1971 and 1972 coefficients are not significantly different from each other, while the 1973 slope is significantly greater than both slopes of the earlier years. Differences in opinion on the marijuana issue lead to meaningful differences in judicial policy following participation of the electorate.

The coefficients of determination (R^2's) reveal a similar pattern. Public opinion explains little of the variance in sentencing in the first two years—8 percent in 1971 and a meager 5 percent in 1972—but, in contrast, a more respectable 19 percent of the variance in 1973. Not only is the structural relationship between public opinion and sentencing significantly greater in 1973, but the overall fit of the data to the regression line is better in the latter year as well.

The *absolute* changes in sentencing provide the most compelling (if somewhat redundant) evidence that the proposition vote, as a clear articulation of citizen opinion, drew forth a response from the courts. A schema for analyzing such changes is presented in Table 2. Examining data from the earlier years, 1971 and 1972, one finds that three sentencing situations are possible relative to opinion: courts are oversentencing, undersentencing, or sentencing consistently with opinion. If response did occur, the first group should become more lenient in its sentencing in 1973; undersentencing courts should become more severe, and courts nearest to opinion should display relatively little change.

Table 3 reports the mean change for each of the three categories of courts.[8] The differential movements accord fully with our expectations. Courts originally oversentencing become discernibly more lenient following expression of public preferences on the marijuana issue, while those originally undersentencing increase the severity of their sentences. The only anomalous finding in Table 3 is the 1972–73 change among courts originally sentencing consistently with opinion. A careful analysis of the individual courts within this category indicates that this higher-than-expected mean is attributable to the change in sentencing policy of two courts. If these courts are removed from analysis, the mean change drops to +5.6.[9]

Table 2. A Schema for Measuring Change in Court Sentencing Behavior

Relative Public Support for Legalization of Marijuana	Relative Sentencing Severity in 1971 or 1972		
	Lenient	Moderate	Severe
High	C	O	O
Medium	U	C	O
Low	U	U	C

Source: Compiled by the authors.

Key: O = oversentencing
U = undersentencing
C = sentencing consistently

The figures in Table 3 take on more meaning when given substantive interpretation. They represent, for example, the difference between a probation of six months and a probation of three years; or the difference between a $50 fine and no fine at all. In short, the changes in sentencing policy had obvious and meaningful impacts on the lives of individuals convicted of possession.

Our initial findings give us reason to believe, then, that the explicit communication of preferences elicited a response from California superior courts. Not only did the cases increase policy agreement in marijuana cases increase significantly after the initiative vote, but also the courts displayed appropriate and differential movement: those courts less in line with opinion in the earlier years showed greater absolute change in sentencing behavior and adjusted their decisions in the predicted direction.

Alternative Explanations. Supportive as the above findings may be, we cannot confidently conclude at this point that superior courts in fact responded to the communication of policy

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coefficients by recomputing the regressions after removing one data point at a time.

[8] For purposes of analysis, we collapsed the nine cells (originally created by dividing the initiative vote and sentencing scores at ± 1 standard deviations) indicated in the hypothetical example above into three categories. This alleviates somewhat the problem of small N's. However, we did look at the patterns across the nine cells. As expected, there was a strong tendency for courts most out of line with opinion to display the greatest change in sentencing, whether the courts were originally over- or undersentencing. We found no evidence that the movement occurred predominantly among rural or urban courts.

[9] Although we were unable to identify factors that directly explain their behavior, these two courts only barely met the minimum number of required cases for inclusion in the analysis. An examination of the remaining categories of courts assured us that the other reported means were not artifacts of deviant cases.

Table 3. Change in Sentencing Policy, by Original Level of Policy Agreement*

Years	Originally Oversentencing	Originally About Correct	Originally Undersentencing
1971–73	–12.2 (N=15)	–1.1 (N=20)	+11.2 (N=8)
1972–73	–16.6 (N=14)	+9.3 (N=25)	+13.4 (N=5)

Source: State of California, *Statement of the Vote* (1972); California Bureau of Criminal Statistics.

*Entries are mean changes in sentencing scores.

preferences. If other factors caused the behavior patterns and relationships just discussed, such a conclusion would be spurious.

Our systemic perspective leads us first of all to account for the complexity of the judicial process. The prosecutor's recommended sentence, negotiated guilty pleas between prosecutors and criminal defense attorneys, and the character of the probation-parole officer's presentencing report, are all part of that process. Moreover, police can and do establish different enforcement policies with regard to laws such as those involving possession of marijuana. Since each of these factors can affect the final sentencing decision (Eisenstein and Jacob, 1977), *the question is whether public opinion (specifically in 1973) has an effect independent of them.*

Although we do not have in-depth information regarding these matters, we are able to add three variables to the original bivariate regression model that might alter our original conclusion. The more complete model is

$$\text{Sentencing Severity} = \alpha - \beta_1 P + \beta_2 C + \beta_3 G + \beta_4 D + \epsilon \qquad (2)$$

where P is the public's policy preference, C the percentage change in the number of marijuana convictions between the sentencing year and the year before, G the percentage of guilty plea convictions, and D the percentage of those originally charged with offenses legally more serious than possession. We assume that C is a reasonable surrogate for prosecutorial or law-enforcement policies. A marked increase in marijuana convictions, for example, would more than likely result from a combined decision on the part of police and prosecutors to crack down on violators. G is used as an indirect indicator of the role of prosecutors in processing marijuana cases. Similarly, we include D in recognition of the fact that downgrading original charges through negotiation may be more common in some courts than others. Patterns of negotiation between prosecutors and defense attorneys, in turn, may condition final sentencing decisions.

Addition of the three variables to our original regression model leads to the equations reported in Table 4. The estimates reveal that none of the new variables has a significant effect on sentencing policy in any of the three years. In every instance the coefficient is smaller or only little larger than its standard error. More important here, our original conclusion about the relationship between opinion and sentencing policy remains intact. Opinion is significantly related to court decisions in 1973 but not in the two prior years.[10]

An additional potential confounding factor is a change in sentencing policy in *all* drug-related cases. A growing recognition of drug trafficking resulting from the activity surrounding the initiative issue itself, for instance, might have led judges or prosecutors in some counties to take tougher stands across the whole range of drug cases. To consider this possibility, we constructed by court and year a mean standardized sentencing score based on four categories of drug convictions other than those related to marijuana.[11] When these scores are tested

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[10] In light of recent research on sentencing (Hagan, 1974), we also controlled for the percentages who (1) were nonwhite, (2) were represented by private counsel, and (3) had major prior criminal records. Since problems of high multicollinearity were identified by an "R^2 delete" test (Kmenta, 1971, pp. 389–90), we regressed the sentencing scores on combinations of two independent variables. The original conclusion remains intact: public opinion has an independent effect on sentencing in 1973 but not in the two previous years. In an effort to avoid ecological fallacy problems, we also considered the impact of all variables on *individual* sentencing scores. Again, the substantive conclusion does not change.

[11] The raw sentencing scores of the four drug offenses were converted into standardized scores. A mean of the standardized scores was calculated for

Table 4. Regressions of Sentencing Severity on Public Preference and Selected Indicators of Presentencing Behavior, 1971–1973

Year	Intercept (α)	Public Preference (β_1)	Percent Change in Marijuana Convictions (β_2)[a]	Percent Guilty Plea Convictions (β_3)	Percent Downgraded (β_4)	$\bar{R}^2$
1971 (N=47)	128.0	–.41 (1.5)[c]	–[b]	.17 (0.5)	.18 (1.4)	.02
1972 (N=45)	100.5	–.34 (1.2)	.07 (1.1)	.14 (0.4)	.20 (1.5)	.02
1973 (N=44)	95.1	–.93 (3.0)	.01 (0.2)	.48 (1.4)	.07 (0.6)	.16

Source: State of California, *Statement of the Vote* (1972); California Bureau of Criminal Statistics.

[a] Variable entered in logarithmic form.

[b] Not included in equation because of resulting reduction in cases.

[c] Figures in parentheses are ratios of the regression coefficients (disregarding signs) to their standard errors.

against our opinion measure, we find no evidence of change in the opinion-policy relationship. In fact, a low relationship in 1971 (R^2 = .09) becomes nearly nonexistent in 1973 (R^2 = .02).[12] Our original findings do not appear to be the product of a general policy change in all drug cases.

Yet another alternative explanation is that the identified change in sentencing was brought about by new judges coming to the bench. Major shifts in personnel would at least raise the possibility that the higher level of policy agreement in 1973 reflects little more than the addition of new judges whose own attitudes were similar to those of the general public from which they were selected. In this instance, the improved relationship between opinion and sentencing policy would be a product of recruitment patterns rather than actual response.

The California Administrative Office of the Courts keeps the most complete information on personnel matters. Although the office's detailed records do not cover the entire three-year period, they do encompass the months of most concern to us.[13] These records indicate that eight vacancies resulting from death were filled in 1973. Ten other judges were appointed during the year, bringing to 18 the total number of new judges added to the bench. Not only is this change in personnel (4 percent) minimal, but equally important, the new appointments were made predominantly in the large urban courts comprised of relatively large numbers of judges. Not one of the appointments was made to a superior court consisting of only one judge (at least this was true of those courts included in our analysis), where the potential impact of a new appointment on overall court sentencing is greatest. In addition, the changes in the sentencing of courts that underwent replacement of judges are not greater, on the whole, than those of courts that did not undergo replacement. Nor are the changes concentrated more heavily in the former courts.

Since the preceding discussion emphasizes that the number of judges remained quite constant during 1973, as it did throughout the entire period, we must consider finally the possibility that caseloads increased substantially during 1973. In particular, caseloads may have increased in the urban courts, which tend to serve publics that are relatively lenient toward marijuana use. Higher caseloads could increase the bargaining out of minor offenses such as possession of marijuana (but see Feeley, 1975), leading to a spurious relationship between sentencing and opinion. We find that caseloads remained strikingly constant throughout the three-year period. Total cases filed per judgeship for the fiscal years 1971, 1972, and 1973 are 1191, 1109, and 1116, respectively (Judicial Council of California, 1975, p. 83). Fluctuations across the individual courts, moreover, are small and apparently random. There is no compelling evidence that caseloads consistently became greater in urban courts.

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each superior court having five or more cases in at least two of the four drug offense categories. Gibson (1976) has employed a similar measure.

[12] The 1972 R^2 is .01.

[13] The California Administrative Office of the Courts does not publish its in-depth information on personnel matters. All figures cited here were made available in a personal communication with the office (March 7, 1979).

The Aggregation 'Problem.' While the data indicate that superior courts responded to the clear expression of public preferences, a crucial question must yet be posed: to what extent do the aggregated mean sentencing scores reflect sentencing *within* courts? The systemic perspective adopted in this article led us to aggregate individual sentencing decisions; but the variability of the mean sentencing scores must be small if our findings are to be valid. Otherwise it could be argued that the mean is simply a product of a wide range of decisions that have no common feature except an arithmetic one.

This concern can be addressed in terms of the coefficient of variation, the standard deviation divided by the mean. A value of 1.0 indicates that the standard deviation is as large as the mean; confidence that no aggregation problem exists increases as the value moves toward zero. Coefficients substantially less than 1.0, in other words, are what we hope to find.

Table 5 reports the mean coefficient of variation by year. The coefficients are both small and strikingly consistent across all three years. And the small standard deviations of the mean coefficients themselves indicate that the coefficients vary only slightly across courts in a given year. Our findings do not appear to be statistical artifacts of data aggregation.

Table 5. Mean Coefficient of Variability by Year

Year	Mean Coefficient	Standard Deviation of Mean Coefficient
1971	.40	.18
1972	.40	.15
1973	.38	.20

Source: State of California, *Statement of the Vote* (1972); California Bureau of Criminal Statistics.

The Absence of Electoral Accountability: A Speculation

To consider more fully the implications of the preceding analysis for democratic governance, we now speculate about one final bit of evidence, namely, that the communication of citizen preferences appears to have elicited government response in the absence of any meaningful electoral accountability of public officials. Consider, first of all, the situation of California superior court judges. These judges have been expressly removed from the vagaries of the electoral process through the institution of staggered six-year terms. Not only are they chosen in technically nonpartisan elections, but also significant numbers in fact obtain their positions through gubernatorial appointment rather than election. Between 1941 and 1959, for example, 97 percent of the superior court judges in Los Angeles County serving for the first time were gubernatorial appointees (Cook, 1967, p. 48). Most significant, sitting judges rarely face "reelection" opposition. In 1972, one of the years included in this study, only 4 of 149 elected judgeships were contested (State of California, 1972, pp. 33–34). Although our data do not allow us to say unequivocally that these judges did not anticipate the possibility of electoral opposition, the likelihood of being challenged was remote at best. And since very few judges even faced opposition in 1972, the changes in sentencing policy clearly were not products of tough challenges in that year's general election.[14]

Although district attorneys are more immediately accountable to the people than superior court judges, highly publicized and competitive campaigns occur only occasionally. For many district attorneys, early decisions not to seek reelection eliminate electoral accountability completely (see Prewitt, 1970). Moreover, cases such as those involving possession of marijuana often are handled by assistant prosecutors functioning as members of large prosecutorial staffs. In 1972, for example, the Los Angeles district attorney had more than *400* assistants (Greenwood et al., 1973, p. 7). Large prosecutorial offices effectively isolate assistant prosecutors from centralized supervision (Eisenstein and Jacob, 1977). Finally, there is no evidence that prosecutors or their staffs became more or less severe in the pre-sentencing process as a result of the initiative vote.[15]

If we are correct in suggesting—and the limitations of our data allow us only to suggest—that government response occurred in

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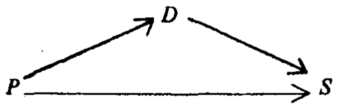
[14] Electoral defeat of superior court judges has become more commonplace in recent years.

[15] To consider the possibility that the expression of public preferences affected prosecutors' pre-sentencing behavior, which in turn conditioned sentencing, we tested the causal model (using the notation of Equation [2]):



In all three years, both links of the indirect path are very weak.

he absence of meaningful electoral accountaility, at least two implications follow. The first s that electoral accountability may not be, as ometimes is supposed, the sine qua non of olitical responsiveness. Noncoercive or volunary response may be more common than is enerally assumed. Indeed, several other recent esearch efforts also report findings indicating hat public officeholders are inclined to repond even in the absence of electoral coercion. ook (1977, also 1979; Kritzer, 1978, 1979) as presented evidence, for example, that fedral district court judges responded to their olitical environments with regard to sentencng in draft cases during the height of the ietnam War. Such judges are appointed for life erms. Similarly, Prewitt (1970) has shown that ay Area city councils compromised predomiantly of volunteers (members not seeking eelection) display notable predilections toward esponsive behavior. Some 35 percent of these ouncils (as opposed to 53 percent of the ouncils comprised of very few volunteers) are haracterized as inclined toward voting with erceived majority preferences. While Prewitt nderstandably interprets his data in terms of he electoral accountability thesis (i.e., elecions do matter), his emphasis leads him to verlook a fundamental question: why should here be any tendency toward responsive beavior when electoral accountability is essenially irrelevant? An answer seemingly lies with ey's observation (1961, p. 538) that a funlamental norm among public officials is "a egard for public opinion, a belief that in some vay or another it should prevail."

The second implication, closely related to he first, is the one we wish to stress: *the ommunication to governmental actors of poliy preferences held by citizens may well be a entral component if not the core of a responive system of government.* The popularity of he electoral accountability thesis has perhaps ed researchers to overlook the role of this oncoercive linkage in democratic governance Luttbeg, 1974, pp. 6–10). Yet it is this linkage hat allows citizens to influence policy making lirectly. Because information can be provided rior to or during the deliberation of policy hoices, the public is not limited, as it is with oting, to a retrospective judgment. It is both nlightening and revealing that Pitkin (1967, p. 32, emphasis added) should conclude her horough treatment of political representation y stating, "The governed must be capable of ction and judgment, capable of *initiating* overnment activity, so that government can be onceived as responding to them." The cardinal enet of democratic theory that government should respond to public opinion, in some measure and in most circumstances, places responsibility on *both* officeholders *and* citizens. Fulfillment of one obligation is predicated upon fulfillment of the other.[16]

But the communication of citizen preferences to evoke desired government behavior does not just happen. Appropriate structures to facilitate communication from citizens to their officials must exist. To borrow once again from Pitkin (1967, p. 232), "There [must] be machinery for the expression of the wishes of the represented...." For the study reported here, such machinery was available—and apparently worked even in the absence of any meaningful accountability of public officials.

This is not to suggest that as long as opportunities exist for citizens to express themselves on salient policy questions, and they avail themselves of such opportunities, public officials will invariably respond. Participation that conveys mixed and conflicting cues about public policy preferences, for instance, may create a situation of "mandate uncertainty" for policy makers (Janda, 1961), and thereby render an "appropriate" response difficult (Verba and Brody, 1970; Karps, 1978). A factor of particular theoretical importance that may condition the participation-responsiveness relationship, and which is not systematically taken into account in our analysis, is the nature of the issue. It is relatively easy for officials to respond to public preferences on an issue such as marijuana use, for whatever the distribution of those preferences, they do not pose a direct conflict with another democratic ideal, namely, the protection and expansion of civil rights and liberties. Opposing legalization of marijuana for personal use is not the same as opposing the admission of blacks into state universities or opposing the presence of homosexuals in public schools.

Given a situation where policy makers are confronted with a choice of *either* responding to prevailing public attitudes *or* upholding civil rights and liberties, two equally tenable response patterns can be hypothesized (below). Pattern A is similar to that identified in this article: response occurs across all levels of expressed public support for the issue. Officials attempt to carry out the public will, regardless of what that will may be. In contrast, pattern B implies differential policy behavior by government officeholders; the level of response varies with the level of support for the issue. Among officials representing publics who are relatively unsupportive of civil liberties issues, the response criterion is secondary to other democratic concerns.[17]

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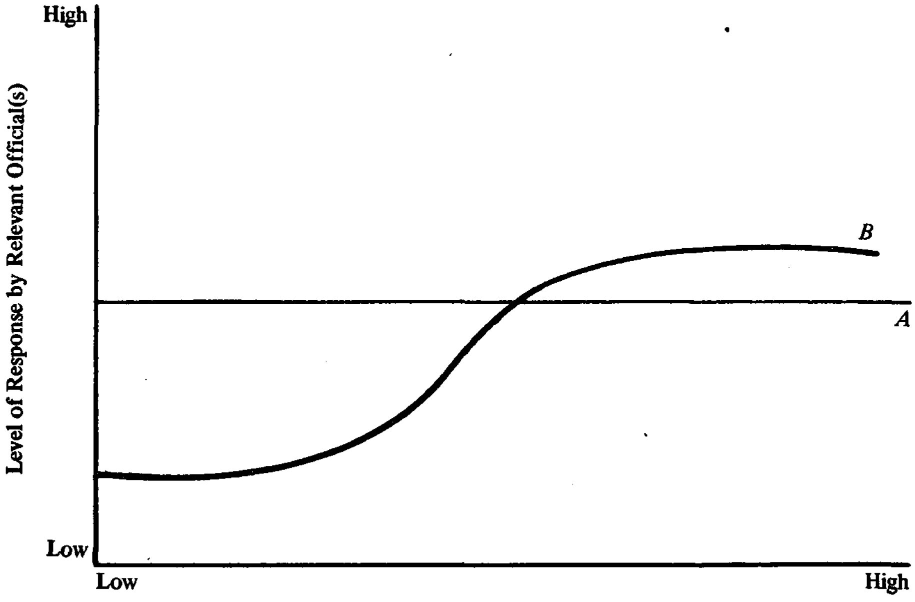
[16] In recognition of the fact that the public often does not hold opinions on issues, or at least does not convey them, Pitkin (1967) includes discretion by government officeholders as a component of political responsiveness. We are using a more restricted meaning of the term here.

While the two patterns represent hypotheses to be tested empirically, they also remind us that, normatively, political participation perhaps should *not* always lead to government response. During the past 15 years, California voters, reasonably representative of the nation as a whole in their opinions, have supported environmental improvement efforts, the extension of voting rights, and bonds to aid poorly financed school districts. But they have also opposed open-housing legislation, supported reinstatement of the death penalty, and rejected efforts to achieve racial balance in the public schools. Not everyone will agree with our assessment that the first set of voter actions is more desirable than the second. But the great disparity in these public opinions recommends that we at least restate the working premise of this study, as expressed in our opening sentence: "Responsiveness is what democracy is supposed to be about—*most of the time.*" Even a quality of government as central to democratic thought as responsiveness must be tempered; the continuing questions are when and how.[18]

Conclusion

The central concern of students of democratic politics long has been, and continues to be, the identification of ways by which the public can shape government decisions to its wishes. Because the voting act can be used to reward responsive officials and to punish errant,



Source: Compiled by the authors.

Figure 1. Alternative Response Patterns on Civil Liberties Issues

unresponsive officials, it is commonly seen as the key mechanism for maintaining a close relationship between government and the people it serves. Our analysis, while it does not dispute the importance of the electoral connection to responsive government, suggests that the direct communication of citizen preferences to public officials is another—perhaps *the*—essential component of the representational linkage.

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[17] The frequency of these two patterns may (and probably *should*) vary across institutions. The expectation of responsiveness is much greater for legislators and executives than it is for judges.

[18] That this statement is more than rhetoric is supported by the fact that researchers have been reluctant to assess institutional performance in terms of responsibility. It is considerably more difficult to determine whether an institution (or official) is acting responsibly than to ascertain whether it is responsive.

Not surprisingly, social scientists and practitioners alike have looked to changes in electoral laws as the panacea for unresponsive government. The findings reported here, although admittedly based on data collected in one state over a short period of time, suggest that more attention be given to institutional arrangements that allow and—it is to be hoped—encourage citizens to communicate their wishes to government officials. Such arrangements should facilitate citizen communication that is direct, concise, and based on a large proportion of the electorate. Encouraging citizens' contributions to a government noted for an elaborate system of checks and balances that works to inhibit responsiveness should foster a close—but not precariously close—relationship between the people and their public servants.

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American Legislative Decision Making and the Size Principle

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This article explains why previous empirical tests of the size principle have failed to produce conclusive results. Particular stress is given to seldom considered aspects of constitutional choice; their implications are explored by means of a simple simulation. Then I propose a test of the size principle and report some initial empirical findings based upon this test.

Despite K. A. Shepsle's observation that "much of the research on and criticism of the size principle is empirical in nature" (1974, p. 516), empirical analysis has so far failed to provide firm conclusions as to its relevance to American legislative behavior. As Hardin has observed (1976, p. 1211):

> There is enormous disagreement over the empirical results. In positive theory, factual claims should be either true or false—not both.
>
> A principal reason for disagreement among researchers has been a general failure to propose what constitutes a fair test.[1]

The purpose of this article is to propose a "fair test" of the size principle. I shall try to explain why it is that previous tests have failed to produce conclusive results; I shall then propose a more satisfactory test and report some initial empirical findings based upon this test.

What the Size Principle Implies

Given a competitive legislature, the size principle implies that the number of votes cast in support of a proposal is predictable. According to Koehler (1972, p. 154):

> In constructing legislative coalitions, [proponents] will direct recruitment to the achievement of minimal winning size and no more. This follows from the assumption that the addition of new members is inherently costly and, therefore, once the coalition is apparently large enough to meet the known minimum requirement of the decision rule, further recruitment would have a negative marginal utility and consequently would be eschewed by rational actors.

In other words, utility-maximizing assumptions generate the prediction that, where other things are equal, the number of votes cast in support of a proposal *will be just sufficient to win and no more.* More generally stated, the size principle states that the number of votes cast in support of a proposal will be determined by the decision rule or rules in effect. Consequently, utility-maximizing assumptions also generate the prediction that the number of votes cast in support of legislative proposals will vary with changes in decision-making rules. Since votes are easily observed and counted, these predictions ought to be directly testable. In fact, they are not, as we shall seek to prove. However, the second proposition may be tested indirectly.

Why Previous Tests of the Size Principle Are Inconclusive

Previous tests of the size principle have sought to test the proposition that the number

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Special thanks are extended to two erstwhile students: Douglas Buchanan for writing the program for the simulation discussed herein and Make Gilson who collected the data described in the last section of the paper. I would also like to acknowledge the help and encouragement given me in the development of this paper by W. T. Stanbury, Ilan Vertinsky, Aiden Vining, and William Zumeta.

[1] Hardin also makes the point in this article that the complexity of the American legislative decision process—number of participants, diversity of their preferences, subjective uncertainty regarding those preferences, etc.—makes a determinate game-theoretical solution to the vote-minimization problem either unobtainable or trivial, but to say that the size principle cannot in this instance be derived by means of an axiomatic game-theoretic demonstration does not mean that it is false, or even that it is irrelevant to an understanding of the American legislative process. Of course, there must be some a priori justification of the size principle if it is to be taken seriously. In this case, however, a priori justification may be found in a somewhat more restrictive set of assumptions. In addition to the choice-theoretic, utility-maximizing assumptions of game theory, we must assume that the supply of support for legislative proposals is competitive. Then, given a set of legislative proposals, each with its proponents, the size principle may be derived via the method of partial equilibrium. See Koehler (1972 and 1975), Stigler (1971), Silberman and Durdan (1976), Peltzman (1976), Abrahms (1977), Fiorina and Noll (1978), and Thompson and Stanbury (1979).

of votes cast in support of a proposal will be no more than sufficient to win. In theory, a direct test of this proposition should be a trivial matter–validation or falsification should require no more than the application of simple descriptive statistics and tests of significance to a set of easily obtainable observations. To falsify or support this proposition, one needs only to

(1) select an appropriate base, or null model, for comparison

(2) identify the minimum voted required given the decision rules in effect, and

(3) determine whether or not the distribution of votes observed is significantly different from (1) in the direction predicted by (2).

As we have noted, counting votes is easy. Moreover, there is a highly satisfactory null model available for comparison–the case in which all possible voting combinations are equiprobable (Hardin, 1976, pp. 1211–12).

Why then is this test not possible? The answer is that one cannot unambiguously specify the minimum vote required given the decision rules and conditions found in real legislatures. Indeed, given the decision rules that actually obtain in American legislatures, it is not possible to make unambiguous quantitative predictions as to either the mean or the variance of the expected distribution of voting outcomes. To show that this is the case, we shall briefly examine the expected consequences of utility maximization under various conditions and decision rules.

Here, five kinds of rules seem relevant:

(1) those which specify a choice rule based upon votes cast, i.e., uninamity, simple majority, etc.

(2) those which specify the conditions under which (1) is satisfied, i.e., a simple majority of those present and voting, a simple majority of those eligible to vote, etc.

(3) those which specify the number of times a legislative proposal may be revised and reconsidered

(4) those which specify the order in which revisions may be considered

(5) those which specify the number of decision sites and the order in which they must be encountered.

Given perfect certainty, supporters of a legislative proposal would put forward the combination of demands and payoffs that would maximize their utility. This, of course, implies that the number of votes cast in support of a legislative proposal is determinate and predictable.

In at least two cases, the expected outcome would be identical to that required by the choice rule. Under unanimity, all proposals voted upon would be given unanimous support; decision rules under (2) through (5) would be moot. Under all other majorities, proposals voted upon at a single site would receive the vote required by the choice rule in effect; rules under (3) and (4) would be moot.

However, we would propose that, where a necessary condition for the enactment of a legislative proposal is its approval by a majority vote at each of several decision sites, the expected outcome will be somewhat problematical. This is the case even under perfect certainty. As will become evident, this claim is particularly important to our argument. Consequently, we shall try to substantiate it.

To begin with, the relevance of this issue to real-world legislative decision making should be evident. At both the federal and the state levels of government, legislation must pass through a complex maze of committees in each of two separate houses of the legislature, must be passed by a majority vote in both houses, and must be accepted by an independent executive.

Furthermore, requiring approval of each of several distinct decision sites was intended by those who designed the American legislative process to constrain legislative decision making. Generally it is acknowledged these intentions have been realized. As Lindblom (1977, p. 151) has observed:

> Separation of powers between legislative and executive, for example, sets each of two groups as watchdog on the other; . . . government is thus a collection of bodies all intentionally crippled because of their otherwise fearful strength. As a consequence all are to a degree . . . impotent where the citizenry asks that something be done.

Perhaps, more to the point, Silberman (1978, pp. 16–17) concludes:

> Naturally, the more difficult the legislative process, the easier it is for a determined minority to block passage. Therefore, in the United States, as opposed to the parliamentary democracies, legislation has usually required a fairly broad national consensus.

That is, requiring approval at several distinct decision sites is similar in effect to increasing the majority required for approval. At a minimum, given a competitive, price-taking legislature, it would raise the price that proponents of a proposal must offer to ensure the enactment of their legislation. The price offered would be determined by the decision site with the highest minimum price required to assure approval of the proposal. As a consequence, even under certainty, the actual vote at any given decision site may not be identical to that specified by the choice rule in effect. (Indeed, except for the case in which supply schedules are identical across all decision sites, it *will* not be at a majority of sites.)

This effect is illustrated by Figure 1. Here price is shown on the vertical axis and is measured in terms of homogeneous units (e.g., disutility to legislative proponents). The number of votes obtained is shown on the horizontal axis. Support supply schedules are shown as S_1 through S_4.[2] Each of these is assumed to be identified with a given decision site, 1 through 4.

The minimum number of votes required for approval at each of the sites is indicated by an asterisk (*). Consequently, the highest minimum price is determined to be P_1, the price required to obtain V_1 on S_1. Note, that at P_1, V_2, V_3 and V_4 are considerably larger than required to obtain approval at sites 2 through 4. For our purposes this is the most important conclusion that can be drawn from this exercise. A second conclusion is that, except where the supply schedules faced by all proposals are identical, the vote actually observed at any given decision site will vary from proposal to proposal. This is so despite the fact that it is the minimum vote required to insure enactment of the legislative proposal.

To investigate the implications of this conclusion further, we devised a very simple computer simulation of a decision-making system with multiple decision sites. This simulation was designed to reflect the relationships between the number and size of decision sites on the one hand, and support prices and observed voting majorities on the other. It was composed of n decision sites or "committees," each with m "members." The members of each

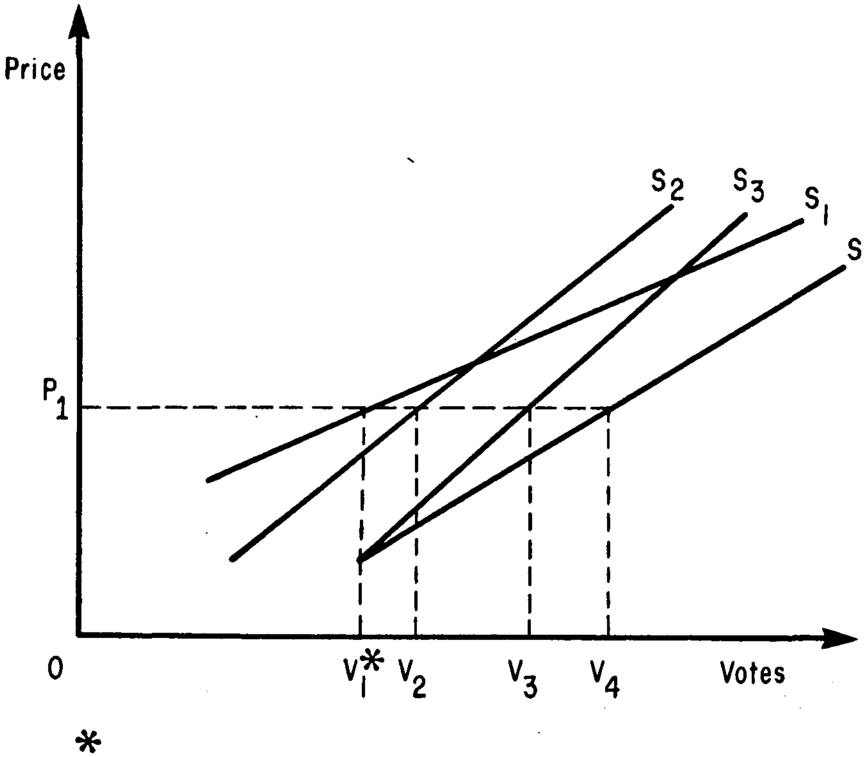

Figure 1. Supply Schedules at Four Voting Sites

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[2] Here, the set of supply schedules shown is assumed to represent the set with the minimum highest, minimum price; subject to this constraint, each supply schedule is, of course, assumed to represent the least costly combination of sacrificed legislation and payoffs required to obtain a given level of voting support.

committee were randomly drawn from a population of 99 "legislators." Committees were approached seriatim. In order for a proposal to "pass out" of committee, it had to receive the support of a simple majority of the members. Members supported the proposal if the price offered equalled or exceeded the member's supply price. If the proposal failed to obtain a simple majority in a committee, the offer price was increased and the cycle repeated until a simple majority was obtained. Legislators' supply prices were normally distributed with the mean supply price set equal to the initial offer price. Here it is reasonable to think of the variance of the distribution as one measure of consensus.[3] The simulation was structured to permit the standard deviation of the distribution to be varied and to allow up to 600 iterations of any run. Here, it is reasonable to think of each iteration as representing a distinct proposal.

In summary, the input variables for the simulation were:

(1) the number of committees (*n*)

(2) the number of members on each committee (*m*)

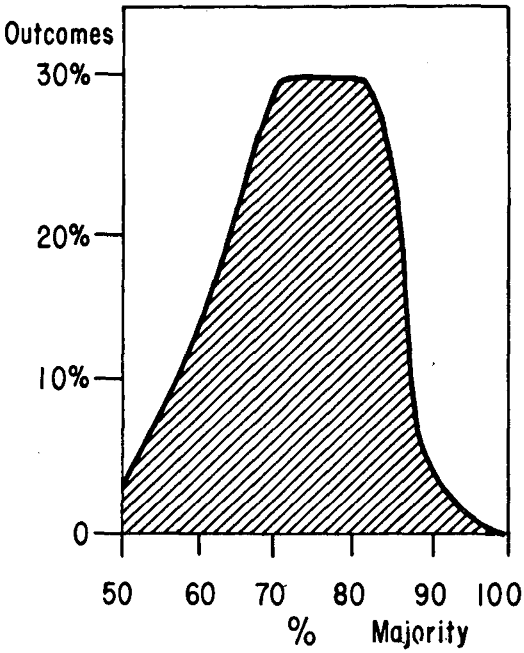
(3) the minimum price required to insure acceptance of the "proposal" by the full "voting population" (equal to population mean)

(4) the standard deviation of the distribution, and

(5) the number of iterations.

The outputs of the simulation were distributions of observed "majorities" and supply prices. Each unique set of input variables produced unique distributions of observed majorities and prices.

Table 1. Sample Simulation Results



The key point of this exercise is that the simulations consistently generated offer prices that were acceptable to large proportions of the 99 legislators. For example, where the number of committees was set at 6, with memberships of 1, 3, 7, 13, 19 and 25 respectively, the initial offer price was set at 50, and the standard deviation of the distribution was set at 5, 400 iterations of the simulation produced the distribution of observed majorities shown in Table 1.

Systematic variation of the input variables also indicated that where the number and size of committees were held constant the mean observed majority increased, but mean price decreased when the standard deviation of the distributilon was decreased. One might interpret this to mean that the effect of multiple decision sites is to increase observed majorities, where the degree of consensus in the legislature as to the merits of a proposal is high, and to increase the size of the payoff required to secure its enactment when consensus is low.

Finally, when the standard deviation of the distribution was held constant, both the observed mean majority and observed mean price increased (at a decreasing rate) with an increase in the number of committees and decreased as committee size was increased.

Under certainty, it would be unnecessary to cycle the proposal through various decision sites until the minimum price needed to achieve enactment of the proposal was reached. Proponents of a policy would know the minimum price required. Consequently, all proposals would be winners and, given the conditions assumed in the above simulation, the prices and majorities produced by the simulation would, in fact, occur. Again, rules under (2) through (4), above, are moot.

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[3] The shape of the distribution might be another. It should be noted here that there is reason to assume that valid subjective estimates of probability distributions for specific legislative proposals may be made. See Adrian and Press (1968), Hinkley (1972), Matthews and Stimson (1971), and Cherryholmes and Shapiro (1969). Furthermore, it is likely that Bayesian techniques might be employed to revise such a priori probability estimates. In this instance survey or quasi-survey techniques could be used to obtain the required a posteriori probability distributions.

This is a determinate outcome. Furthermore, the mean and the distribution of expected majorities are predictable. However, accurate prediction requires prior specification of:

(1) the number of decision sites

(2) the number of participants at each decision site, and

(3) the standard deviation of the legislative supply function.

Only the last of these presents problems and, if we can assume certainty on the part of the participants in the decision-making process, we can assume certainty on the part of the analyst.

The problem here is that certainty is a wholly unrealistic assumption. Of course, if there were no costs associated with delay, one might imagine a system in which proposals were recycled at successively higher prices until approval were given, as in the simulation, in which case, the size principle would be binding only on the final, successful vote. Therefore, so long as the analyst attended only to winning votes, the size principle could be directly tested.

But delay is costly (Buchanan and Tullock, 1962, pp. 63–91; Breton, 1974). For this reason legislatures establish rules governing the number of times a proposal can be redefined and reconsidered and the order in which redefinitions are considered. This means that both the proponents of a proposal and legislators must face considerable uncertainty. Furthermore, owing to the absence of readily understandable measures of price and quantity, it is not possible to assume a Walrasion process of *tâtonnement* that would produce a perfect equilibrium despite such uncertainty. Under uncertainty,

> each action that may be chosen is identified with a distribution of potential outcomes, not with a unique outcome. Implicit in uncertainty is the consequence that these distributions of potential outcomes are overlapping. It is worth emphasis that each possible action has a distribution of potential outcomes, only one of which will materialize if the action is taken, and that one outcome cannot be foreseen. Essentially, the task is converted into ... selecting an action whose potential outcomes distribution is preferable; ... there is no such thing as a maximizing distribution (Alchian, 1950, p. 215).

That is to say, one can talk about minimizing the vote required to win, subject to a given probability of winning, or of maximizing the probability of winning, at a given price, but one cannot simultaneously minimize the vote required to win and maximize the probability of winning.

This point may be illustrated by the simulation described above. If each iteration of the simulation is taken to be an alternative reformulation of a single proposal, given a known minimum price and degree of consensus, the simulation tells us that the probability that any given combination of possible winning combinations of sacrificed legislation and payoffs is the minimum winning combination is less than 2 percent. Alternatively, we might say that for a given combination of sacrificed legislation and payoffs, the probability that it is a winning combination is equal to or less than some specified percent. In the example shown above, a price of "51" would have less than 2 percent chance of success. (See, however, footnote 3.)

The conclusion that where foresight is uncertain vote minimization is meaningless does not imply that reasonable probabilistic predictions of voting distributions are impossible. In order to make such a prediction it would be necessary to know:

(1) The number of decision sites at which approval must be given. This information should be relatively unambiguous.

(2) The number of votes required at each decision site to secure approval of the proposal. This also should be unambiguous where the choice rule specifies a number, more ambiguous where number depends upon the number of participants present and voting.

(3) The standard deviation of the supply schedules. It is reasonable to assume some knowledge of the degree of consensus in the legislature on certain issues; this knowledge can be increased by survey techniques. Furthermore, there is no reason to assume

that in real legislatures, committee assignments are random.[4]

(4) The risk preferences of proponents of the policies considered. Here unfortunately a number of equally plausible assumptions may be made, e.g., that the consequences of winning and losing are asymmetrical, perhaps, because payoffs have to be made regardless of the outcome. In that case, proponents would err on the side of caution. Alternatively, it might be noted that cognitive theorists tell us that people tend to treat uncertain outcomes as if they were certain (Fischhoff, Slovik and Lichtenstein, 1976; Fishburn, 1973; Kogan and Wallich, 1964). In that case, pessimists would err on the side of caution; optimists on the side of imprudence.[5]

To this list should perhaps be added the degree of monopoly power exercised by participants. For example, power to set rules as to the order in which proposals are considered, etc., might be interpreted as a unit veto. This factor should also be accounted for in making predictions of expected vote distributions.

However, it is not possible that a direct test of the size principle based upon such a prediction would be conclusive. We could be no more sure of the test than of the product of the certainty of our knowledge of 1 through 4. Since we cannot be certain of this knowledge, direct tests of applications of the size principle to American legislative decision making must necessarily be inconclusive.

An Alternative Test of the Size Principle

Owing to the complexity of the relationships influencing roll-call voting behavior, I have argued that a direct test of the size principle cannot be conclusive. However, social scientists have long recognized the utility of the method of partial equilibrium as a means of decomposition of complex systemic relationships. Its utility rests on the fact that

> in cases where the equilibrium values of our variables can be regarded as solutions to an extremum . . . problem, it is often quite possible regardless of the number of variables involved to determine unambiguously the qualitative behavior of our solutions in respect to changes of parameters (Samuelson, 1947, p. 21).

Since the size principle is clearly based upon an extremum formulation, it is reasonable to think that an alternative test might be obtained via the method of partial equilibrium.

Indeed, this is the approach that Koehler took in his test of the size principle, albeit informally. Koehler (1972, p. 155) hypothesized that "changes in party agreement would not produce changes in the corresponding voting agreement which instead will remain fixed and minimal." That is, he hypothesized that

$$\left.\frac{\partial V}{\partial P}\right|_{\substack{R = r^\circ \\ U = u^\circ}} = 0 \qquad (1)$$

where

V = size of the majority cast on roll-call vote
P = party majority
R = decision rules
U = uncertainty as to support supply schedules.

This hypothesis may clearly be derived from a formal statement of the size principle. Furthermore, since it is reasonable to assume that U is random over time and not correlated with P, and since it is known that R has remained stable over time, time series analysis of congressional roll-call votes should provide a satisfactory empirical test of Koehler's prediction. Basing his conclusion upon an analysis of 11 time periods, Koehler demonstrated that $\frac{\partial V}{\partial P}$ is not $\neq 0$.

This outcome is clearly consistent with what the size principle would lead us to expect. Nevertheless, while a contrary result would have cast strong doubt on the size principle, Koehler's findings do not provide positive evidence of its validity. As Hardin (1976, p. 1212) quite correctly has noted: "What [Koehler] actually demonstrates is that the average size of winning coalitions on contested roll-call votes does not correlate very well with the size of the dominant party's majority."

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[4] Most legislators receive the committee assignments they request and their requests are correlated with the interests of their constituents. See Rohde and Shepsle (1973), Shepsle (1975), and Cohen (1974).

[5] If people ignore uncertainty and are pessimistic (i.e., they assume the worst will happen), they will behave *as if* they were minimizing their maximum possible loss (mini-max regret). If they are optimistic, they will behave *as if* they were maximizing their maximum possible gain (maxi-max). Either decision rule has been shown to be consistent with observed political behavior. See Ferejohn and Fiorina (1974 and 1975). Unfortunately, one cannot simply assume optimistic or pessimistic expectations; Samuel Bodily, for example, has shown (1978) that for one class of "risky" decisions about 50 percent of the population are consistently optimistic in their expectations, 25 percent are consistently pessimistic, and 25 percent are consistently neutral.

To test the size principle proper, we should want to determine whether or not a change in a key parameter results in a predicted change in the solution value. In this case, we should ask whether or not decision rule changes produce expected changes in observed majorities. For example, it follows from the size principle that

$$\frac{\partial V}{\partial M}\Bigg|_{\substack{R = r^\circ \\ U = u^\circ}} > 0 \qquad (2)$$

where

M = the required majority
R = other decision rules.

Furthermore, it follows from the utility maximization assumptions that underlie the size principle that if M is increased, proponents of a policy will wish to reduce U; hence:

$$\frac{\partial^2 V}{\partial M^2}\Bigg|_{R = r^\circ} > 0. \qquad (3)$$

Unfortunately, evidence that $\frac{\partial V}{\partial M} > 0$ would not be very persuasive evidence of the size principle. Confirmation of a counter-intuitive prediction provides for stronger support than does a demonstration of the obvious. Common sense leads us to expect that observed majorities will increase if the required majority is increased. Demonstration that $\frac{\partial^2 V}{\partial M^2} < 0$ would appear to constitute a far stronger proof of the size principle, since it is not at all clear that this is what common sense would lead us to expect. Regrettably, $\frac{\partial V}{\partial M} > 0$ may be subjected to an empirical test, but $\frac{\partial^2 V}{\partial M^2} < 0$ cannot. Here, the problem is the lack of appropriate data. To test the first of the propositions, we need only to identify a change in M. For example, California's Proposition 13 requires a two-thirds majority to pass state tax bills, whereas before, simple majority vote in each house of the California legislature was sufficient. After a few years, time-series analysis will tell us whether or not this change results in an increase in observed majorities on tax bills. But a similar test of the second proposition is not possible. Additional parameter value data are needed and they are not available. Unfortunately, required majorities are not systematically varied for the convenience of political scientists. Cross-sectional analysis might provide the needed variation in parameter values, but unless other systematic variations in parameter values are held constant, the resulting coefficient estimates likely will be biased. Data gathered from within a single legislature would certainly be unsatisfactory, owing to the fact that required majorities are covariant with types of legislation. Cross-jurisdictional data might prove satisfactory, but it seems likely that the results of such a test might be confounded by variations in the size of legislatures, sophistication of policy advocates (which could be correlated with the size of the state), and so forth.[6]

Similar problems are encountered when other decision rules are considered directly. This does not mean, however, that a satisfactory test of the size principle is infeasible. I argue that it is not necessary to address changes in decision rules directly. I have claimed that an increase in the number of decision sites is equivalent to an increase in the size of the majority required. The test I propose is based upon the assertion that prior approval at alternative decision sites is the equivalent of a reduction in the number of decision sites. That is, if the number of decision sites determines the price that must be paid to ensure enactment of a policy proposal and, therefore, determines the size of the observed majority, we would expect that prior approval at other decision sites would have the effect of reducing the price offered by proponents of a policy and, therefore, of reducing the size of the majority observed in a roll-call vote. For example, legislation initiated by the executive should produce smaller majorities than legislation initiated in a substantive committee of a particular house of a legislature, since executive approval is assured. The same logic should apply to votes on actions which do not require the concurrence of the other house or to situations where the other house has already passed the legislation.[7] Hence, it is proposed that

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[6] See, however, Koehler (1975).

[7] See Strom and Rundquist (1977); see also DiPalma (1976).

$$\frac{\partial V}{\partial A}\bigg|_{\substack{R = r^\circ \\ U = u^\circ}} < 0 \qquad (4)$$

here

A = prior approval of the executive.

A Test of this Proposition

To test this proposition, it was first necesıry to hold constant the type of legislation. his was accomplished by focusing upon a set f spending bills (authorizing legislation) en:ted in California during August and Septemer of the second half of the 1974–1975 gislative session. Legislative proposals which iled on any of the following criteria were xcluded from the sample:

l) the proposal had to be contested, trailer and supplementary legislation was excluded from considerations;

2) the proposed expenditure had to exceed $1 million;

3) the governor had to take a position on the proposal prior to first reading before Assembly Ways and Means Committee.

wenty-seven enacted proposals met these ıree criteria.

The next step was to determine whether or ot one could reject with any degree of onfidence the hypothesis of no difference in V etween proposals supported and opposed by ıe governor. Furthermore, it should be noted ıat the predicted direction of the change, i.e., rior approval produces a decrease in the size of ıe observed majority, appears somewhat coun:r-intuitive. One might suppose that the suport of a popular Democratic governor would ıcrease the majority given a proposal by an verwhelmingly Democratic legislature.

The statistical test used was quite simple. I ombined the observed roll-call vote in each of ıe two houses and executive position data in a ngle regression according to the following ıodel:

$$Y_i = \hat{\alpha} + \hat{\beta} X_i$$

here

Y_i = the observed majority on proposal i;
X_i = the position of the governor (0 if opposed, 1 if supported) on proposal i.

If it is assumed that the error terms for different observations are normally distributed and uncorrelated, one can formulate an F test to tell us whether the coefficient (β) of the independent variable is different from zero. This test was performed on both sets of data. The parameters were estimated using ordinary least squares. The resulting estimated equation for the Assembly data is:

$$Y_i = 66.5 - 3.87X_i$$
$$r^2 = .27$$
$$F_{1,26} = 3.97, \text{sig.} > .90.$$

The resulting estimated equation for the Senate data is:

$$Y_i = 29.54 - 2.04X_i$$
$$r^2 = .12$$
$$F_{1,26} = 4.49, \text{sig.} > .95.$$

That is, we can be 90 percent certain the $\beta \neq 0$. Note also that $\hat{\beta}$ has the predicted sign.

Despite the fact that the average observed majority greatly exceeds the minimum required vote in both houses (41 in the Assembly and 21 in the Senate), I suggest that these results provide positive support for the size principle. Of course, the data used in this test may be somehow biased or I may have simply selected an idiosyncratic sample. Strong positive support of the size principle must await further empirical confirmation of these results. However, testing the size principle is not the primary purpose of this article. My primary purpose was/is the design of a "fair test" of this notion. It is my belief that I have achieved this purpose.

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COMMUNICATIONS

ON ARTICLES

Comment on the Holmes and Nichols Exchange (Vol. 73, March, 1979, pp. 113–38)

TO THE EDITOR:

I would like to comment on the dialogue in the March 1979 *Review* between Stephen Taylor Holmes' "Aristippus in and out of Athens" and James H. Nichols' reply, "On the Proper Use of Ancient Political Philosophy."

Holmes argues in his article that the Greek ideal of polis, as both society and government, is one of an undifferentiated political whole made of various parts of society, and that such a political ideal no longer applies, and should not be applied, to the modern state, in which life is differentiated into spheres of competing concerns: church versus state, bourgeois capitalism versus government. Holmes then argues that, a dichotomized society being the case, any application of Greek "holism" to the modern state will result in something anachronistic, retrogressive and, it is clearly implied, morally undesirable. Further, he posits, this is exactly the error of totalitarian states and hence, in his view, Marxist and neo-Marxist thought.

Though Nichols responds to some of these points, when answering the totalitarian issue, he restricts himself to saying that totalitarian governments are not guilty of anachronistic Aristotelianism because they in fact violate its premises, and Marxism, too, rejects the sober moderation and political ends of Aristotle's teaching.

What Holmes fails to manifest, and Nichols fails to answer directly, is the charge that Marxism or neo-Marxism is synonymous with totalitarianism. It is obviously not, and yet wording in both articles in various places suggests that it is. Surely no one as seemingly astute as Holmes or Nichols could believe that Marxism must inevitably trace the path of Stalinism, that "hope for a radically different future" must necessarily "justify brutal and murderous means" (Nichols, p. 130). Yet both writers are explicit in their bias toward these conclusions. It is enough to suspect that either ignorance of Western-socialist, neo-Marxist volumes or capitalist ideological myopia is at work here. In either event, it is unsettling to think that these two scholars were incapable of even imagining a distinction between Marxism and coercive government. (For readers with similar tunnel-vision, this distinction has been recognized by individuals as diverse as Ralph Miliband, Charles Bettelheim, Michael Harrington, Michael Best, William Connolly, Leon Baradat, Roy Medvedev, Santiago Carillo, Franz Marek, Giorgio Napolitano, Jean Ellenstein.)

A second point to affirm is a consideration Nichols makes and, in my reading, Holmes does not substantively refute. This is the fact that even the most "differentiated," most modern states are componentially integrated and in that sense holistic. The modern state is, indeed, a "whole encapsulating its subordinate parts," and "a command center which orders society and makes it cohere" (Holmes, p. 135). The notion that it is only fascist or "communist" governments which are totalitarian is the truly anachronistic idea in Holmes' essay—not the Greek model. Nichols defends the "holism or totality" of state culture when he argues, "Apolitical thinking in contemporary liberal society suffers from the same defect of superficiality; it fails to recognize the ultimate dependency of individual freedoms (and of the quasi-autonomy of social interaction contexts) on the political regime of liberal democracy" (Nichols, p. 132).

It is a fallacy for Holmes to reason that because some parts of the modern state may be differentiated they are not also coercively coordinated and manipulated. The distinction between capitalist power and law is viewed by many as illusory. Though religious values and forces may be divided from legal rights and legislative bodies/parties, the rupture is more theoretical than empirical. Too, *where, in theory,* religious virtue may seem a struggling underdog to material pragmatism, it is only because the modern state has in many respects succeeded in transforming secularism and empiricism into a holistic civic culture of its own, with priority over, and eliminating the need for, a religious alternative. Psychologists and sociologists have long documented the manipulative socialization that occurs in schools, fraternal organizations, clubs, workplaces, and through intra-class relationships, and the media. These factors indicate the modern state is far more holistic and undifferentiated and, in *that* sense, totalitarian, than it might pretend. (Recall discussions of economic/social/political holism in the works of, for example, C. Wright Mills, G. William Domhoff, Herbert Marcuse, Louis Althusser, Paul Sweezy, Michael Parenti, Ira Katznelson and Mark Kesselman, G. David Garson.)

To answer Holmes directly, there is very little which is "beyond politics." To assume there are many areas where democratic government does not need to be is to assume that those areas do not affect us unless we choose them to. This is laissez-faire nonsense. If one of the normative functions of politics is to pursue and guarantee justice, then politics must often define as its territory those public concerns controlled privately.

Democratic neo-Marxists may believe politics, economics, religion, and socialization are all holistically and undifferentially related in any society, and they may believe that there is a more just economic solution than capitalism available, requiring comprehensive political planning. But that prospect is not, by any stretch of the imagination, a more totalitarian solution than the modern Western states have already found.

ROBIN GASS

Chaffey Community College

Reply

TO THE EDITOR:

Ideologies, of course, may bolster political regimes, but they cannot be "synonymous" with them. As Marx said in the *18th Brumaire,* the rhetoric of classical republicanism lent an aura of legitimacy to the activities of the Committee of Public Safety, but it was only one factor among many. In my original article, I had very little to say about Marxism. Nothing I did say, moreover, implied the fashionable Parisian insight that "Marxism must inevitably trace the path of Stalinism." In fact, the most I would want to claim is that Marxism—like various extreme right-wing ideologies—has historically had a high potential for being (mis)-used to *legitimate* non-democratic regimes. Several of the illustrious souls Robin Gass parades before us have written something similar.

But Gass is not simply wrong about the implications of my article. Because of her lack of clarity about the connotations of the old organicist metaphor, she cannot even present her own position in a coherent way. For example, it is conceptually absurd (leaving aside the empirical issue) to say both that there is *no distinction* between capitalist power and law, and that the capitalist class coercively *manipulates* the law. Likewise, it is unintelligible to assert (without further qualification) both that politics *should* take over crucial social sectors now controlled by "private" enterprise, and that politics and the economy are *already* "holistically and undifferentiatedly related."

Without presuming to unravel Gass' numerous inconsistencies, I would like to speculate on their source: even though she is vaguely aware that a "more just" society would require *more differentiation* (for instance, between law and capitalist power), her conceptual pre-commitment to diffuse social "wholeness" forces her to deny explicitly what she implicitly affirms.

S. T. HOLMES

Harvard University

ON BOOK REVIEWS

Comment on Wertheimer's Review (Vol. 73, March 1979, pp. 226–27)

TO THE EDITOR:

In his review of my *Punishing Criminals,* Alan Wertheimer suggests that I am "not aware of some of the difficulties in [my] argument" and "make some glaring errors." He may be right. His generosity, perhaps excessive, must have prevented him from substantiating the "glaring errors" I made. Instead, he charges me with errors I did not make.

Quoting my "conduct prohibited by law is wrong" (p. 9), Wertheimer comments: "This is either tautologically true (if he means . . . legally wrong) or false." But I continued: "To engage in it is to commit an offense. Legal guilt is incurred. . . ," i.e., the "tautological" interpretation was intended in this section, which explicitly pointed out that acts "not intrinsically wicked . . . whatever their moral quality" may be offenses. However, even if my "wrong" is misread to mean "morally wrong," it still does not become "false." Unless he contends that conduct prohibited by law is never morally wrong, Wertheimer must have meant—but did not write—"sometimes false (and often irrelevant)."

Wertheimer quotes (p. 16), "Since we all benefit from even the worst social order . . . we all owe society some allegiance in exchange" and comments that I have not shown either the benefit or that it creates an obligation. I didn't, because I attributed this view (which has been around since Socrates) to others, adding: "The idea loses what explanatory value it has . . . when treated as an actual contract rather than an . . . obligation which many people reject. . . ." I do not believe the obligation can be shown.

Wertheimer writes that I "argue that society has an obligation to punish criminals because it has promised to do so," and adds: "A person to whom a promise is made normally can cancel the obligation. . . ." I explicitly repudiated the view Wertheimer attributes to me and pointed out that the threat to punish criminals is a promise not to them but to the law-abiding who indeed can "cancel" it. Criminals cannot. They receive no promise but a threat.

Wertheimer quotes, "Utility must prevail in making laws, whereas the distribution of punishments must be retributive" (p. 25), and charges that I do not tell "just why retributivism would be wrong" in making laws. I didn't tell because I don't think it is. I merely explain why it should not prevail: "Acts are prohibited on pain of punishment because the prohibition reinforced by the threat is meant to keep people from doing what is prohibited," i.e., the threat of punishment has a utilitarian purpose—which does not make a retributive purpose "wrong." I did explain, however, why the distribution of punishments to specific persons (whether or not useful) must be based on their past crimes and not on future behavior: "No amount of utility then could make the incarceration of an innocent person just, nor is utility required for the punishment of guilty persons" (p. 26). I cannot here reproduce the argument leading to the conclusion. Wertheimer is entitled to ignore it, but I cannot find any basis for his misreading.

Wertheimer writes, "[van den Haag] errs again when he argues that juveniles should be held responsible for their crimes because 'the victim of a 15-year-old mugger is as much mugged as the victim of a twenty-year-old mugger' " (p. 174) without mentioning my next sentence: "The need for social defense or protection is the same." I meant to indicate that the need for social defense is imperative enough to discontinue the immunity given juveniles. I may have misled Wertheimer into attributing the belief to me that the harm done suffices to make the doer responsible for it. I do apologize and assure Wertheimer that I am acquainted with the notion of *mens rea.* So is every careful reader of *Punishing Criminals.*

ERNEST VAN DEN HAAG

New York Law School

Reply

TO THE EDITOR:

Ernest van den Haag denies that *Punishing Criminals* contains the "glaring errors" I attributed to it. After reading van den Haag's response and after reexamining the text, I stand by the claim that the argument does, in fact, contain several errors. The reader will have to determine whether or not they are "glaring."

Van den Haag does argue that acts which are prohibited by law are *legal* offenses, whatever their intrinsic moral quality. He distinguishes between acts which are intrinsically evil (*malum in se*) and "non-intrinsic evils" or "bad because prohibited" (*malum prohibita*) (p. 9). He claims that I have misread his argument in thinking that he means to say that prohibited acts are *morally* (as opposed to legally) wrong. If so, then what is the point? To say that acts which are prohibited by law are legal offenses is tautologically and trivially true. A definitional claim cannot establish the state's moral right to punish law violators. Moreover, since the words "bad," "wrong," and "offense" normally carry at least some moral force, I do not think I have misread the argument. I believe that van den Haag means to argue that because the state is morally entitled to make laws and because citizens have an obligation to obey the law, acts which are otherwise not immoral become so by virtue of being prohibited. Perhaps so. But then van den Haag needs a moral argument which has not been supplied. He correctly notes that I should have written that his claim "conduct prohibited by law is wrong" is *sometimes* false. I hasten to point out, however, that if it is sometimes false, then it also is not true that "conduct prohibited by law is wrong."

Punishing Criminals states: "True, since we all benefit from even the worst social order . . . we all owe society some allegiance in exchange" (p. 16). According to the book, the idea which loses "explanatory value" is the idea of an implied promise or "contract," particularly an implied promise by the criminal. But, contrary to the impression given by van den Haag's letter, my review did not state that he held a "contract" or "consent" theory of obligation. I argued that he held a "receipt of benefits" theory (which I believe is quite different) and that on this theory he has not shown that all do benefit from the worst social order. More importantly, even if all receive some benefits, he has not shown that those who are systematically oppressed receive benefits which are greater than the costs imposed on them. I find it surprising that van den Haag should now state, "I do not believe the obligation can be shown." Surely he must believe that some obligation can be shown, or it would seem that the moral legitimacy of punishment is in deep difficulty.

Van den Haag also now denies that he believes society has an obligation to punish criminals in virtue of its promise to them. I am pleased that he no longer wishes to defend this view. But *Punishing Criminals* does:

> Debts must be paid in the first place because they are owed, because one has promised to pay them. Retribution must be paid because it is owed, because it has been threatened, and a threat is a (negative) promise. The payment of debts (or of retribution) fulfills an obligation undertaken in the past (p. 14).

The book goes on to say: "Society has obligated itself by threatening. It owes the carrying out of its threats" (p. 15). Now it is true that van den Haag correctly argues against the view that the criminal exonerates himself by being punished, by "paying his debt." But the whole point of his discussion is to say that it is *society's* debt (not the criminal's debt) which is at issue. Indeed, he argues that society has an obligation to the criminal and "law-abiding citizens *as well"* (p. 16, emphasis added). Thus, I restate my point: society cannot create an obligation to punish the criminal simply by threatening (promising) to do so, unless it has a pre-existing right to make the promise. Perhaps it does. But van den Haag has not shown that it does.

As my review suggests, van den Haag adopts a popular rule-utilitarian position in holding that "utility must prevail" at the legislative phase and that "justice must prevail in the judicial phase. . ." (p. 25). I suggested that he does not show why legislative retributivism would be wrong. He now says it would not be wrong—"I merely explain why it should not prevail." Let me rephrase the point. Why must utility *prevail* at the legislative phase? Why would a thoroughgoing retributivism be false? *Punishing Criminals* contains no answers to these questions. Moreover, if utility is the proper underlying objective of punishment, one needs a complex *argument* (not mere assertions) which shows why punishment should not be distributed on a (sophisticated) act-utilitarian basis. Assertions to the effect that "justice must prevail in the judicial phase" are not sufficient. The incompatibility of retributivism and utilitarianism cannot be so easily defused.

Punishing Criminals states: "Children surely should not be held responsible for their conduct to the extent adults are" (p. 173). The book goes on to ask: "But should we regard sixteen-year-olds or even fourteen-year-olds as children?" (p. 173) and then points out that the "need for social defense or protection [against juveniles] is the same" (p. 174). Now if there are good reasons for treating children as less responsible than adults, the fact that the need for social protection from juveniles is the same (it may be greater) as the need for social protection from adults does not and cannot establish the appropriate degree of juvenile responsibility. Nowhere did I suggest that van den Haag is unacquainted with the notion of *mens rea.* It simply has not been adequately analyzed nor invoked in this context.

ALAN WERTHEIMER

University of Vermont

Comment on Rosenthal's Review (Vol. 73, March 1979, pp. 321–23)

TO THE EDITOR:

In a recent review of my book, *The French Popular Front: A Legislative Analysis,* Howard Rosenthal raises a very important criticism that merits a response. He questions the statistical methodology that I employ in analyzing roll-call votes, and suggests that roll call analysts such as myself must "shoulder the burden of proof" that their techniques are appropriate to the underlying reality.

The "proof" that I have developed was presented in two articles, "A Reevaluation of Alternative Methodologies in Legislative Voting Analysis" and "The Definition and Measurement of Similarity among Roll-Call Votes" which were published in *Social Science Research* (1975) two years before my book appeared. This material was not repeated fully in the book because it seemed to me that a brief presentation of the technique and a greater concentration on the subject of the study itself was more appropriate in that case. Incidentally, Rosenthal will be interested to find that in those articles are developed: (a) the underlying assumptions about voting behavior, including a treatment of abstentions which is different from the assumption he made in criticizing me, and (b) a precise analysis of the relationship of Guttman scaling to principal components and an indication of when each is to be preferred. Needless to say, on this basis I reject the criticisms that Rosenthal makes of my methodology, which were based on his reading of one article which he admits does "not pertain directly to roll call-by-roll call analyses." Whether one accepts my approach to legislative analysis or not, I should not be accused of not having thought through the issue of technique.

PAUL WARWICK

Simon Fraser University

Reply

TO THE EDITOR:

I am delighted that Paul Warwick has introduced his 1975 *Social Science Research* articles as additional evidence. These articles beautifully illustrate the central point of my review—that mindless application of a statistical black box (e.g., factor analysis, Guttman scaling) will generally fail to recover the underlying behavioral space of a legislature.

Warwick (1975, pp. 363–65) establishes a hypothetical two-dimensional legislature where legislators are points and roll calls cutting lines. "Yeas" and "Nays" lie on opposite sides of a line. Those exactly on the roll call's line abstain.

What do his computations show?

(1) *Principal components factor analysis can readily give the wrong dimensionality.* Although the true dimensionality is two, Warwick generally finds three or four dimensions! (Similarly, unidimensional data [p. 255] needs three components.) The excess dimensionality immediately implies that the behavioral space is not recovered correctly.

(2) *Even when the dimensionality is correct, the space may not be recovered.* For example, two distinct roll calls, with legislators in all four quadrants formed by their intersection have identical (to the two decimal places reported) factor loadings. (See roll calls 2 and 13 in the last columns of Table 3 [p. 370].) Also, even within the same "scale," roll calls that are relatively distant may have more similar factor scores than roll calls that are relatively close. (Compare roll calls 11, 13, and 15 in the D2 + D3 columns of Table 3 [p. 370].)

(3) *If a number of roll calls have small angles of intersections, certainly likely in applications to real data, Warwick acknowledges the inappropriateness of principal components* (p. 371) *and* as well, both the Q and gamma *Guttman methods* (p. 378).

Warwick does suggest (p. 263) that the unwarranted dimensions of principal components are useful in identifying blocs of legislators. But, in the case of one dimension, blocs fall out just as readily from a simple scalogram, such as Table 4 (p. 255). As to higher dimensions, Warwick presents no direct evidence that factor scores usefully cluster legislators as they appear in the original two-dimensional space.

Finally, as to my "reading of one article," that seminal piece by Richard J. Morrison was enough to suggest the impoverished foundations of most roll-call analyses, including Warwick's. Not surprisingly, Warwick's own computations for roll call-by-roll call matrices confirm the lessons of Morrison's analytical work on legislator-by-legislator arrays. If statistical techniques are not grounded in explicit models of political behavior, the black box is likely to be a garbage truck.

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Comment on Hoole's Review (Vol. 72, December 1978, pp. 1509–10)

TO THE EDITOR:

In his recent review of Stuart A. Bremer's *Simulated Worlds* (1977), Francis Hoole issues a number of criticisms which are pertinent not only to the book, but also to the enterprise within which the volume must be placed: socio-political global modeling. Most specific points in the review are well taken. Yet the thrust of Hoole's conclusions is, if misinterpreted, at cross purposes with Hoole's avowed support of the "goals and approach" involved in developing computerized models of global social, political, and economic relations.

Hoole is concerned about the adequacy of the various verbal, mathematical, and flow-diagram expositions of the model in the volume. The difficulty of reconciling these presentations was an impediment to developing an "understanding" of the various meta-components of the model, including: (1) Inter-Nation Simulation (INS) theory (Guetzkow, 1963) and (2) the adaptive and self-monitoring decision-making algorithms Bremer developed, such as the international trade mechanism, diplomatic conflict, budgetary activity, and the formulation and monitoring of national goals vis-à-vis strategic military positions, domestic political stability, and economic growth. Bremer's presentations sought to highlight the substantive aspects of the theory without obscuring essential details of the model or bogging down the interested reader in complicated technical materials. This was accomplished with elan and most limited resources.

Hoole's dissatisfaction reflects the likelihood that, in general, presentation of simulation materials cannot stand alone. It is necessary to document all phases of such projects in great detail at all levels, from the theoretical design to the computer code used to implement it. The verbal material and the flow diagrams should be combined with a listing of the program (as appeared in Bremer's dissertation). Particularly for someone as familiar with simulation methodology as Hoole, or someone who wishes to utilize the program for research or teaching, access to the program is essential. Participants at the recent International Institute of Applied Systems Analysis' Sixth Global Modeling Conference (Vienna, 1978) stressed the need for adequate documentation in the modeling field (Donella Meadows' estimate was that adequate documentation required half of the hours and money in global modeling projects). The substantively updated program (SIPER4.0) has been thoroughly documented in the listing with statements which tie the functional program statements to both a computerized cross-referenced glossary and to Bremer's various written discussions of the model (available upon request).

Although tracking down subsequent uses of scholarly material is customarily outside the obligations of a reviewer, one wishes Hoole had sought some of the additional materials available. World 3 was similarly critiqued for omitting technical details in early publications. Yet the excellent documentation of the Meadows' team was precisely what permitted other scholars to examine and probe their model so carefully (see, among others, Bremer, 1978, p. 52). In the long run, Hoole's objections will be similarly undercut by the availability of extensive documentation for SIPER4.0.

The validity of the simulation model is tied to the initial development of INS. When INS was created, the data movement in international relations was an infant, if not but a twinkle. This led Guetzkow to use prototypic variables (and nations) in constructing the model. Bremer (1976, p. 309–15) has discussed extensively how using a prototypic strategy has hindered efforts to validate INS and SIPER. Yet in spite of the unresolved nature of the validity issue, INS-based theory and SIPER as well, by virtue of their prototypic constructions, have made possible considerable rigorous and systematic scholarship in international relations. Nor should we forget that appreciable scholarship has been devoted to validating the INS model on which *Simulated Worlds* is founded (Guetzkow, 1968).

As to the question of "deductions . . . of interest" (Hoole, 1978, p. 1510), the simulation itself is a systematic, rigorous, and internally consistent concatenation of a large number of hypotheses drawn from the scientific literature of international relations (see Wallace, 1977, pp. 241–42). Both INS and SIPER have aimed at cumulative theory construction. Such work has been facilitated, in part, by the use of prototypic variables. Much of the rigor of the construction has been self-consciously shaped by applying the mathematical and logical demands of the simulation format to the hypotheses drawn from the literature. The operation of the simulation yields a series of consequences—which are testable. With respect to how interesting these deductions are, I should note that a major concern underlying the operation of the model deals with the joint, interacting influences of political, economic, and strategic considerations upon the allocation of resources by national political entities. Basic research on these issues will interest scholars for some time to come, as they encompass the interrelationship of "high" and "low" politics.

Hoole does not ask the critical question regarding the Bremer volume and the model it expounds: where does it fit in the effort to construct systematic and rigorous mechanisms with which to better "understand" global relations? It is worth noting that the Bremer effort preceded the widely popularized Club of Rome world-modeling projects. More importantly, until recently these latter efforts have asserted the primacy of economic formulations in building models to aid global understanding of salient phenomena. By exclusion, they indirectly argued for the triviality of political and strategic concerns. It is only in the late 1970s that global modelers are coming to the position advanced much earlier, fundamental in Bremer's contribution: that economics is by itself an inadequate framework for global modeling efforts, and somehow sociological, political, and strategic variables and relationships must be systematically and rigorously included in global models (Ward and Guetzkow, 1978). That is the significance of the Bremer work, and the tradition from which it emerges: it successfully builds an operational model of global affairs which explicitly incorporates the politico-economic-military aspects of global relations.

Even by comparison to the more widely

known models, Bremer's work fares remarkably well in its formulation and in its reportage. To be sure, it must be substantively updated—and work has been under way for some time. What is crucial, however, is that the global simulation/modeling thrust beyond the boundaries of economics not be lost in a barrage of criticism which fails to place Bremer's achievement within the larger context.

The irony is that had *Simulated Worlds* been published sooner and received the attention it deserves, perhaps the global modeling field would have been forced to confront the sociological and political vacuum of their models sooner. Might then the world modelers have incorporated crucial facets in their work at earlier stages, yielding quite different insights into our collective futures?

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Reply

TO THE EDITOR:

I accept your invitation to respond to Michael D. Ward's quarrel with my review of Stuart A. Bremer's *Simulated Worlds.* And I shall reply with brevity.

Ward and I will just have to agree to disagree respectfully on the contribution of *Simulated Worlds.* There is nothing in Ward's communication which makes me want to change anything in my review of Bremer's book. I do want to wish Ward well in the work in which he has been engaged in recent years in building on Bremer's model. As I noted in my review, "I agree with the goals and approach, but not with the execution of the research [reported in *Simulated Worlds*]."

FRANCIS W. HOOLE

Indiana University

Comment on Vaillancourt's Review (Vol. 72, December 1978, pp. 1491–92)

TO THE EDITOR:

Pauline Vaillancourt's review of *Must Canada Fail?* which appeared recently raises serious questions both about the limits of fair comment in a review, and about the editorial responsibilities of the book review editor.

There is, of course, much that I disagree with in Vaillancourt's assessment of the book, most of which can be reasonably debated. My concern, however, is with one passage which reads: "Some of the articles manifest a certain degree of sensitivity to Quebec's present position on independence and sovereignty-association. Others are so narrow and prejudiced as to border on racism."

This allegation is in no way substantiated in the review; nor does the reviewer identify which of the 17 authors fit into her two categories. As a result, every one of the contributors stands accused, in the eyes of your readers, of prejudice and racism. Those are not terms to be used lightly; they constitute very serious allegations which are deeply disturbing to me and my colleagues. They are, we believe, totally without foundation.

They are also charges which are difficult to refute, since they are so general and unspecified. It is, however, significant to note that in the many reviews, in both French and English which have appeared previously, not one reviewer has made a similar statement. Indeed, the majority of reviewers have underlined the depth of understanding of a sympathy with political developments in Quebec in recent years.

The duty of the book review editor, surely, is to ensure that reviews meet minimal standards of fair comment, and that such serious criticisms which inpugn the integrity of the authors not be allowed to appear in print unless fully supported.

Since the allegation in Vaillancourt's review is so erroneous, I believe that both reviewer and editor have seriously damaged the personal reputations of the authors, and that an apology is required.

RICHARD SIMEON

Queen's University

Reply

TO THE EDITOR:

The authors of *Must Canada Fail?* are clearly entitled to have criticisms substantiated. My remarks were not trivial or inconsequential. I therefore regret that I did not document them more completely both because of the imposed limits of length and because I did not wish to embarrass particular individuals personally. But Richard Simeon exaggerates when he charges that I have impugned "the integrity" and damaged "the personal reputations" of all those who contributed to this book. On the contrary, because of the manner in which the "objectionable" phrases are worded, none of the contributors is accused of racism.

Many of the articles in this book are sensitive and perceptive. Others are not. As a reviewer I have the responsibility of alerting the potential reader to what I consider to be unacceptable in some of these articles. I could not gloss over biased, preconceived ideas or partial analyses. Neither could I exonerate judgments based on the assumption that English Canadian institutions or culture are superior to those of the Quebecois. It is of course possible that the remarks to which I refer were unintentionally offensive. Nevertheless they must be judged on their objective content and not on the basis of the authors' motivations. I would like to emphasize that not all of the articles in the book are involved, as the following examples will indicate.

The provincial government of Quebec is portrayed as scheming and manipulative (p. 59), unreasonable (p. 68), lacking "good will" (p. 69), naive (p. 189), "beguiling" (p. 189), opportunistic (p. 297), guilty of publishing statistics which are "perhaps inaccurate" (p. 251) and of a problematic character (p. 257). These are personal opinions, not facts.

It is indirectly implied that Quebec nationalism is xenophobic (p. 129) and socialist (p. 22), engaging radicals, political activists and the left wing (p. 23). Quebec nationalism could become "illiberal and intolerant" at any time, we are told (pp. 129–30). But throughout the book English Canadian nationalism is viewed as quite understandable, a normal phenomenon.

A distinctly paternalistic attitude permeates certain articles. If Quebec nationalism is intolerant, it is contended, this is a "consequence of economic inferiority and uncertain capacity to shape major social institutions" (p. 130). If Quebec were to achieve independence, it would end up weaker than it was within confederation (p. 242). "The best security Quebec can have is to remain within Canada" (p. 83).

We are told that other Canadian provinces, rich in resources, would have no trouble functioning as independent countries (British Columbia, p. 70; Alberta, pp. 82–83; the western provinces as a group, p. 104). But Quebec's project for sovereignty-association is unfeasible (pp. 103, 257). Foreboding and veiled threats characterize some articles. There is no support for Quebec independence, we are told (p. 103). The other provinces will oppose it (p. 69). In some cases warnings are explicit: violence would be unleashed (p. 103), rapid polarization could result (p. 189), civil war such as that in Northern Ireland is possible (p. 189). Armed intervention (p. 205) from the United States, perhaps even France, is contemplated (pp. 189–90). It will not be easy to avoid "large-scale violence or repression" (p. 203). Questions and innuendo create an impression of impending disaster should the Quebecois people vote for sovereignty-association (p. 26). This kind of analysis lends credence to the most unthinkable scenarios (pp. 199–201, 219–21) such as the expulsion of anglophones from Quebec (p. 129) or a coup d'état by leftists replacing the democratically elected Parti Quebecois (p. 200).

The plausibility of these scenarios is called into question by the public opinion pool data presented in another article in the book. While not willing to grant Quebec independence, Canadians are willing to make accommodations (p. 30). There is no fear for the future expressed by the majority of the Canadian population (p. 30), and certainly no support for the idea of forcing Quebec to stay in confederation (pp. 32–33). One author maintains that English Canadians would not accept economic association with a sovereign Quebec (p. 297) but the data indicate (p. 34) that economic union would be approved by the majority of the Canadian population!

The historical development and the probems of various regions in Canada are presented n part 2, but information about Quebec and he centuries of oppression experienced by the uebec people, essential for a complete undertanding of events in Canada today, is absent. n the contrary, one author suggests (p. 76) hat Quebec has received too much from the est of the provinces in the past! Another rticle outlines strategy for the federal govern-ent. Written in Machiavellian language, it akes no effort to present a balanced perspecive (pp. 177–85).

I believe it is clear from these examples that one of the articles in this book is racist. hether or not those cited are "so narrow and rejudiced as to *border* on racism" must be ecided individually by each reader. I hope that hese examples serve, in some way, to clarify y own evaluation for *Review* readers.

PAULINE VAILLANCOURT

niversity of Quebec, Montreal

Editor's Note

The *APSR* apologizes to the editor and uthors of *Must Canada Fail?* for inattentive diting of the review of the book appearing in 'ol. 72, December, 1978, pp. 1491–92. We efer specifically to the statement by the eviewer that certain articles in the book "are narrow and prejudiced as to border on cism" (p. 1491). It should be noted that *PSR* policy does not permit rejoinders by thors of communications.

Comment on Nau's Review (Vol. 73, March 1979, pp. 329–30)

O THE EDITOR:

Henry R. Nau's review of my *Technology, orld Politics and American Policy* (1977) ontains inaccuracies which significantly distort e point of view of my book. I am writing this tter to set the record straight, and I would ppreciate your publishing it.

Nau alleges that I do not understand that luralism is not only a consequence of the olitical process, but also "a basic value ensur-g individual choice and safeguarding against arrow and inflexible national choice." This llegation is not true.

I recognize and emphasize the value of luralism in a number of places in the book. My point of view is perhaps best represented by the following statement: "The American system of government is based on the separation of powers and concomitant pluralism, whose purpose is to safeguard freedom. The value of pluralism built into our system has been proved time and again, Watergate being only one important example. However, on top of the pluralistic characteristics of our governmental system, the impact of technology has overpluralized Washington and the entire nation. Excessive pluralism in the U.S. government presents a serious problem inasmuch as it impedes the government's ability to control the impact of technology and thus create conditions necessary for the enjoyment of freedom" (pp. 310–11).

Nau states that, for the United States, I allegedly favor "a more rational method of decision making in which 'the interests of various groups would be given due consideration. . .' but 'in the last analysis, the decision would be based objectively on what is good for the nation as a whole and not on the power, vociferiousness, or similar attributes of a particular constituency.' " Henry Nau accurately describes my definition of the "rational" (as distinguished from the "constituency" or purely pluralistic) method of decision making, but its acceptance for the United States is not what I am in favor of. I believe there should be a *balance* between the rational and the constituency decision making. Again, to quote from my book: "The task, then, is not to choose one or the other, but to strike the most suitable balance between the two for a particular period of history. Moreover, since each method has its own advantages and disadvantages, a qualitative distinction is appropriate: For a given balance the best elements of each method must receive emphasis" (p. 199).

Because of excesses of pluralism in our society, I argued in favor of moving the balance in the direction of the rational method. However, I emphasized that "this new balance need not, and should not, reduce the pluralistic nature of our system to the point that its ability to safeguard freedom is jeopardized. But it would be expected to eliminate those excesses of pluralism, largely induced by technological impact, that seriously impede the ability of the government to function effectively and to introduce desirable changes" (p. 311).

Nau uses my argument for a U.S. national purpose of a high civilization (or "a virtuous, enlightened society") in which all the people would share as a point of departure for his discovery of "the obvious shortcomings" of my book. Using the aforementioned inaccuracies to support his contention, Henry Nau implies that my argument is authoritarian in approach. This is not true. As I pointed out on pp. 297 and 312, there are indications of grass-root support in American society for objectives of a high civilization. If my analysis of the future is correct, in time an increasing number of constituencies are likely to emerge in support of this objective. However, an effective presidential leadership, fully compatible with the democratic process, would be required.

VICTOR BASIUK

Arlington, Virginia

Comment on Cannizzo's Review (Vol. 72, December 1978, pp. 1533–34)

TO THE EDITOR:

Cynthia A. Cannizzo's review dismisses my *Field Theory Evolving* as "an affront to philosophers of science, an outrage to statisticians [which] will be irrelevant to nearly all international relations scholars and political leaders." This invites response.

Regarding the book's contents, Cannizzo only notes (incorrectly) that it is "an attempt to answer past critiques and show how field theory has grown and changed." The omission of a summary is critical, for it would reveal weaknesses in her subsequent technical criticisms.

Field Theory Evolving attempts to develop, elaborate and test a social field theory of international relations. The theory is that international behavior occurs in a dynamic field (like a free market) of diverse force potentials involving national interests, expectations, perceptions, and dispositions; these forces are imbedded in international meanings, values and norms; which forces influence what behavior depends on the location of nations in a sociocultural space-time defined by their economic, political, demographic, and cultural attributes, among others; and these forces lie along the distance vectors between nations in this space-time. That is, international differences and similarities (such as rich-poor gap, power disparity, ideological distance) explain much of international relations.

In *Field Theory Evolving* I try to define this theory mathematically and subject it to critical, empirical tests. This raises complex theoretical and operational problems for which widely useful solutions are developed. My tests included numerous factor and canonical analyses, and varied, widely useful empirical results. Indeed, the theory aside, using about a millio pieces of data, *Field Theory Evolving* probabl presents most comprehensive general empirica findings on national attributes, behavior, an their linkages. As for field theory, the bool shows that it has face validity and interna consistency, and is important, operationalizable, testable, and well confirmed by th empirical data.

Now, for Cannizzo's technical points:

1. Field Theory Evolving *ignores "mathematical implications."* No specifics. A questionable point: chapters do explicitly derive th theory's mathematical implications, includin the component and canonical models, statu theorems, the appropriate empirical forecasts and the relationship between field and attribut theories.

2. *It uses "regression analysis while denyin the regression model."* No specifics. Since *Fiel Theory Evolving* uses the canonical model t test the theory, perhaps the reviewer confuse canonical and regression analysis (a *subcase* o canonical analysis for one dependent variable)

3. *It confuses "statistical with substantiv significance."* No specifics. I am sensitive to thi distinction, which I elaborated in my *Applie Factor Analysis* (1970), and I maintained i throughout *Field Theory Evolving.* Indeed, th concluding chapter explicitly evaluated th theory's substantive versus statistical significance.

4. *The theory "is fundamentally unrealistic."* This is supported by four criticisms:

4a. *Operationalization.* Noting that facto analysis operationalizes the international sociocultural space-time, the reviewer asks, "Why don't the resulting factors resemble theoretical dimensions laid out by Wright?" (Quincy Wright posited an "analytic field" in his 1955 *A Study of International Relations.*) The answe is that my theory is not Wright's (and his factors were intuitively posited), although there are *some* conceptual similarities. The reviewe confuses similarity in name with similarity in theory.

Cannizzo also asks uncomprehendingly why if factor analysis is proper it delineates many odd combinations of variables. True, for some smaller factors. But the largest attribute-factors clearly define wealth, power, size, politics, stability, density, and culture—hardly odd. Anyway, this is operationally irrelevant: the question is whether factor analysis is mathematically appropriate to the theory.

In any case, Cannizzo notes negatively that factor analysis is linear and offers a "quick glance" through the scientific journals which reveals: "Most of the important relationships in international relations are curvilinear and multiplicative." First, she incorrectly views linearity as only a first-degree equation, unaware that it also covers the combination of terms in an equation. E.g., $y = a + bx + cx^2 + dx^3$ is a curvilinear equation, but a *linear* combination of terms. Now, field theory assumes a *linear space of equations whose terms are linearly combined, as does factor analysis* (making it appropriate for operationalizing this space). That is, field theory and factor analysis span many multiplicative (e.g., $y = a + bx + cxz$) or curvilinear ($y = a + bx + cx^3$) relationships.

Second, Cannizzo's point is irrelevant anyway. Linearity in *Field Theory Evolving* is theoretical, not operational. Field theory assumes linearity (in the space), and its appropriateness depends on the empirical tests. The book presents 51 tests on different nations, years and data. These show that field theory explains about half the bilateral behavior of, for example, Burma, Brazil, China, Egypt, the U.S., or USSR. By contrast, the reviewer submits her "glance" at the journals, leaving implicit her alternative, a priori *theory* of nonlinearity.

4b. *Time. Field Theory Evolving* entails social time, which the reviewer asserts creates unpredictability. This claim is also a priori, and ignores the book's many positive, empirical tests.

Additionally, Cannizzo argues that attributes change more slowly than behavior. "Hence, attributes cannot possibly account for short-term fluctuation in national behavior." A surprising point, given, say, the effects on foreign behavior of the Iranian revolution or China's new leadership. Regardless, she misunderstands the theory and methodology. By theory behavior and attributes are standardized, thus equating behavior and attribute variance.

Even were variables unstandardized, however, she would still be incorrect. Even though attributes change slowly, their *combined linear effect* may produce surges in behavior (e.g., small cycles may in combination produce high peaks or a flat trend). Anyway, attributes produce behavior only through individual perceptions, interests, and expectations: a slight change in attributes can, through its perception and relevance to interests, trigger sharp change in behavior.

4c. *Feedback.* After then inconsistently noting "that many of the rapid, large, and/or discontinuous changes in attributes result from behavior," Cannizzo argues that ignoring such feedback distorts the results. But in *Field Theory Evolving* one-way causation is a theoretical assumption—an ordering framework (as are cause and effect in Kant's view) for comprehending the hidden complex of international forces. Its appropriateness (as with linearity) depends on the theory's efficacy and empirical confirmation, which the book shows are good. The reviewer provides no explication or evidence for her contrary *theory.*

4d. *Variables.* "Predictor variables are another major source of distortion in field theory." To wit: "the factor analysis" of 236 variables. First, this was done in my *Dimensions of Nations* (1972), not in *Field Theory Evolving* (two chapters use indicators from it). Second, Cannizzo mistakenly claims the analysis is on the dyadic level. Third, the book presents not one, but many factor analyses, such as of bilateral behavior for the U.S. in 1955, 1960, 1963, and 1965, for China during 1950–1965, and for the USSR in 1960 and 1965.

Cannizzo also criticizes the omission of system- or individual-level variables and, finally mentioning an actual fit to data, asks, "What good is a theory that can statistically account for 80 percent of the variance in national behavior when it does not specify the causal mechanism?" Apparently, she dislikes the theory's generality. Fine. She wants to know the intervening causal mechanisms. Also understandable. But *Field Theory Evolving* is only one book. As the book points out, volumes 1 and 2 of my *Understanding Conflict and War* (1976–) provide what she asks and could have been consulted.

In total, her points 4a–4c supporting field theory's alleged "unrealistic" nature are technically uninformed or intuitive, ignoring the book's many positive, empirical results.

Points 1–4 above constitutes the review. To return to the resulting condemnation:

5. Field Theory Evolving *"is an affront to philosophers of science."* The above points are methodological or theoretical; Cannizzo mentions no philosophical blunders or even errors. She simply denounces the book's character. Now, if developing a formal theory, deriving operationalizations and methods from it, attempting to falsify it empirically over 50 times, and assessing its elegance, richness, operationalizability, importance, testability, and confirmation is an affront to philosophers of science, so be it. However, *Field Theory Evolving* is consistent with this philosophy (e.g., Popper, Nagel, Hempel), which originally oriented me towards social science. My debt is footnoted throughout the book.

6. Field Theory Evolving *is "an outrage to statisticians."* The reviewer's points concern approach and research design and cannot support this destructive censure. No gross statis-

tical errors or flagrant mistakes are mentioned. I can only say that *this* statistician is not outraged.

7. *It "will be irrelevant to nearly all international relations scholars and political leaders." Field Theory Evolving* is not for political leaders, nor traditional scholars. It is a theoretical-empirical work, to be judged as a contribution to the *science* of international relations.

To summarize, Cannizzo's points are incorrect, improper, or irrelevant. They show unfamiliarity with the book's contents, with theory and theory testing and with the relevant mathematics and methods. Her condemnation of the whole work must therefore rest on ignorance.

R. J. RUMMEL

University of Hawaii, Manoa

Reply

TO THE EDITOR:

This reply to Rummel's response is, of necessity, short, as was my original review of *Field Theory Evolving.* With space at a premium one can never document and substantiate arguments in the manner Rummel would like. I have only two points to make.

First, if my review is as irrelevant and as full of errors as Rummel alleges, why did he bother to respond? Does he not trust our colleagues to see through it as he did? I think he does not. Such a belief on my part is substantiated by the number of people who commented to me that they felt my review was sound and right on target. One of those people added, however, that he thought my review was perhaps a bit too harsh. That criticism I will accept.

Second, it seems to me we need to put this argument in perspective. This is not a case of Rummel versus Cannizzo, but a part of the fundamental and raging debate that divides scholars in macro-quantitative research on conflict and international relations. This debate has apparently "progressed" so far that emotionalism and rhetoric outweigh scholarly interchange and that neither side can correctly understand the work of the other. If I misunderstood parts of *Field Theory Evolving,* Rummel also misconstrued what I was saying in my review. I strongly advise readers of these exchanges to take a look at *Field Theory Evolving* through their own peculiar epistemological lenses and come to their own conclusions regarding the relevance of this book.

CYNTHIA CANNIZZO

Ohio State University

EDITORIAL NOTE

Carl Beck passed away on October 24, 1979. The profession lost a leading scholar, the University of Pittsburgh lost a dynamic and forceful director of its Center for International Studies, and the *APSR* lost a friend and supporting member of the Editorial Board. Carl Beck lived at least two lives in one. He had unbounded enthusiasm for the academic enterprise in all its forms. For Carl, there were no insuperable barriers. "Let's do it" was, perhaps, his most common reaction to any proposal. He launched careers, he stimulated and supported research and writing, he made connections. What was started by Carl will not pass with his leaving but it will be done with less joy.

BOOK REVIEWS

Political Theory and Methodology

A Little White Lie: Institutional Divisions of Labor and Life. By Robert E. Agger. (New York: Elsevier North-Holland, 1978. Pp. xiv + 193. $14.95.)

Robert Agger's book exemplifies a barely nascent tendency that may be understood as humanistic political science. "Humanistic" here, as in psychology, would indicate roughly a focus on human beings as autonomously shaping, and assigning significance to, their experience, through their capacity for integrative self-awareness. The "little white lie" that concerns Agger is the modern model of people as aggregates of discrete, objectivizable needs that are met through corresponding discrete institutions. He calls this model "institutional consciousness."

Agger identifies the problem in thinking that needs are discrete as caused by our treating values as equivalent to needs, and consequently as also discrete. However, he asserts, values are actually holistic "human experiences toward which . . . people are oriented" (p. 16), so that conflating them with discrete needs is an error. He concludes that needs may be discrete after all, but now his key point is that values are not.

Similarly, Agger objects to conceiving of "everyday life" as separate from, and residual to, institutional contexts. He asserts that everyday life is actually the total experience of holistic persons, so that "despite what people think, all life is everyday life" (p. 96). Therefore what we think are discrete institutions are really imbedded in everyday life; "discrete institutions do not exist" (p. 13).

These conclusions rest on Agger's assertions that values and everyday life are "actually" holistic. He conceives these to be ontological points, but formulates them as definitions. Since such formulations can be refuted simply by stipulating other senses for the terms, Agger's propositions appear only as attempts to "demonstrate" that discrete needs and institutions "do not exist" by making the conclusions definitionally true.

The exposition is persistently weakened by such confusions about the relations between concepts and phenomena. For instance, surely it is the orientation to experience, and not the experience itself, that constitutes value. In particular, Agger badly wants a concept of the relation between ideas as theoretical and as constitutive of social reality; he too often resorts to locutions of the form, "what's real today isn't really what's real."

Agger also tends to value conceptual over material realities. His main attack on the idea of discrete needs is in fact directed only against the specific distinction of material from mental needs; his implicit intent emerges as countering the latter's relegation to a "higher but lesser" place in human life. Thus he praises Marx for understanding "production" to include activity satisfying any human need, but criticizes him for sometimes restricting "production" to economic activity.

There is, however, a cogent intention behind Agger's exposition. His implicit thesis is that there is a level of human experience not encompassed by reductionist or objectivizing analysis, and that we must take this level into account if we are to understand institutions and human values adequately. His argument should thus focus not on how institutions and values are defined, but on what account we give of them: that what we do designate as values and institutions are not the kinds of things that institutional consciousness supposes. He could then concentrate on showing that his alternative accounts illuminate just those points that institutional consciousness mystifies. As it is, one remains unconvinced that conceiving institutions and values as discrete is really the problem, rather than, say, conceiving them as suprapersonal or hierarchical, aspects which he treats as secondary.

Agger's intention therefore ultimately emerges as a critique of ideology, specifically of institutional consciousness as the ideology of technocratic elitism, and as a challenge to the corresponding practice. His analysis suffices to show that people in institutional settings are not reduced to roles, but remain "total persons" who experience the settings in terms of their holistic values. Institutions are thus in fact shaped by the purposes and actions even of those whom institutional consciousness defines as "passive" clients and operatives.

Agger therefore concludes by proposing, not that specialization and structure be abolished, but that existing roles be reconceptualized to provide explicitly for, and to foster, this participation. He briefly develops some examples of, and strategies for, doing so. This concept of "opening institutions" goes a step beyond previous formulations and makes a useful contribution.

RICHARD S. BETH

Southwest Missouri State University

Democracy, Consensus and Social Contract. Edited by Pierre Birnbaum, Jack Lively and Geraint Parry. (Beverly Hills, Calif.: Sage Publications, 1978. Pp. 361. $15.00, cloth; $7.50, paper.)

At worst, these articles offer perfectly competent surveys: Hannu Nurmi of public choice models of the state; Percy Lehning of property rights in Rawls and Buchanan; Christine Mironesco of game theory. And, with the forgiveable exception of John Gray's broodings on Rawls, the pieces are refreshingly unpretentious. In all, the collection would be as useful as any for introducing advanced undergraduates to modern political theory.

In professional terms, these articles are less significant than the enterprise producing them. This volume, together with Brian Barry's collection *Power and Political Theory* (London: Wiley, 1976), announces the formation of a new circle of political theorists meeting annually under the aegis of the European Consortium for Political Research. What may eventually come of it is still uncertain, but this collection offers some clues and much hope.

Theirs is political theory in the broadly analytical mode. The continentals involved speak the language of Anglo-American philosophy, but with interesting accents. They, and the circle as a whole, are wonderfully free of American preoccupations. Only Pierre Birnbaum could be accused of flogging an utterly dead American horse, the "functions of apathy" literature. And of the 16 essays only three address the Rawls-Nozick debate dominating the American scene at the time. Of these, only John Gray's attack on Rawlsian moral epistemology (and especially on the "reflexive equilibrium") counts as a real contribution. Hillel Steiner's essay fails to live up to the promise of its marvelous title, "Can a Social Contract Be Signed by an Invisible Hand?"

Inevitably, the British serve as the initial mainstay of the group. Their contributions to the present volume serve as useful ballast. David Miller explores tensions between liberal conceptions of democracy and justice, showing that whereas early liberals preferred to resolve the tensions by forsaking democracy (restricting political equality), modern liberals have instead resigned themselves to building considerations of needs into the original meritocratic notion of justice. Jack Lively exposes the consensual presuppositions of pluralistic conflict models: if the state is merely an impartial arbiter, then it embodies shared community values; if political entrepreneurs are instead engaged in bargaining, then everyone must at least acquiesce in the initial distribution of bargaining power and concede the neutrality of the political market. Geraint Parry surveys the views of the standard Great Men on the problem of how much and what sort of things a good citizen needs to know, concluding that even the passive role of being governed demands considerable political sophistication. Even Graeme Duncan's rendering of the tired old individualist-communitarian refrain offers some interesting commentary on the problem of fitting small participatory communities into a "total society" organized, by necessity, around very different principles. All these are utterly solid, if rather uninspired, pieces.

Being more polished, all the facets and flaws of the British essays are pretty apparent. The continental contributions are often more fascinating because, as uncut gems, we are unsure of their ultimate cash value. Often the vignettes are far better than the overarching theories, as in Uli Windisch's discussion of the interplay between class and collective consciousness in Swiss xenophobic movements protesting migrant workers or in Valais, an isolated mountain community managing to combine clan-based local politics with class-based national politics. In other essays, the theory is still tantalizingly tentative. Herman van Gunsteren, for example, wants to recast the notion of citizenship so as to satisfy both Althusser and volunteeristic Enlightenment liberals simultaneously by enquiring into what stands behind what liberals must regard as arbitrary exercises of individual wills. It is an improbable task, to be sure, but in light of van Gunsteren's earlier and equally improbable success (in framing a Wittgensteinian public administration), perhaps it is best to reserve judgment. Far the most impressive is Elias Berg's terse analytic treatment of the notion of self-determination, decomposing it into the two reflexive notions of "self-choice" (meaningful participation) and "self-relevance" (issues at stake must be relevant to one's projects and concerns). Threats to self-determination are treated under the general rubric of "perceptual distortion," either of the essence of the self or of causal relations determining outcomes; and the threats stem either from willful manipulation or from systemic mystification. This is the sort of high-quality, original theory we might hope to see continuing among this group.

ROBERT E. GOODIN

University of Essex

Democracy in Deficit: The Political Legacy of Lord Keynes. By James M. Buchanan and Richard E. Wagner. (New York: Academic Press, 1977. Pp. xii + 195. $14.50.)

Economists Buchanan and Wagner are concerned with the interaction between economic ideas (specifically those attributable to John Maynard Keynes) and political institutions. Their basic thesis in *Democracy in Deficit* is that the Keynesian theory of economic policy has inherent biases toward inflation and a growing public sector when applied within the institutions of political democracy. Part of this bias is owing to problems in the Keynesian model as perceived and implemented by academics and politicians; the authors argue that Keynes' disciples, if not Keynes himself, paid little attention to monetary policy and did not recognize public debt as a burden on present or future taxpayers.

But the more serious difficulty involves the goals and strategies of elected officials and bureaucrats. It is usually politically profitable to foster economic expansion through government intervention in the economy. But few votes are to be garnered by applying the brakes to reduce inflation or to decrease government spending. The result, over time, is a tendency toward continued inflation, soaring deficits, and increased size of government. Even these are not sufficient to eliminate unemployment; as inflationary finance comes to be anticipated, private capital investment is diverted to government spending, transfer payments, and investments which depend on continued inflation for their profitability. In their phrase, "Ploughs, generating plants, and fertilizer would be sacrificed for TV dinners purchased by food stamps" (p. 67).

The basis for this disheartening conclusion (and the plan of the book) is threefold. Buchanan and Wagner first explore the intellectual history of the gospel according to Keynes—a fascinating discussion of Keynes' elitist assumptions concerning public policy, the unique circumstances of the Great Depression, and the ways in which this new paradigm worked its way through the academic mainstream into the media and the consciousness of politicians. The second section of the book briefly surveys economic policy making in the United States from 1932 to 1976, including an insightful discussion of stagflation, the demise of the Phillips Curve, and current economic issues.

In the third section, Buchanan and Wagner build on their previous writings to present a public-choice model of Keynesian political economy in practice. They argue that under conditions of political competition, the goals of elected politicians, bureaucrats, and monetary authorities converge to produce easy budgets and tight money, whereas the Keynesian prescription for inflation would call for tight budgets and easy money. Voters' rational ignorance about taxes, spending, and the true causes of inflation perpetuate this system, with disastrous results. Since voters are unlikely to learn enough economics to evict the politicians responsible for such deficit financing, Buchanan and Wagner favor passage of a constitutional balanced-budget amendment. Although they admit this may not be effective in containing government growth, they are highly critical of alternative remedies for inflation (central planning, full-employment budgets, or budget balance over the business cycle) for both political and economic reasons. But they neglect to subject their balanced-budget proposal to similar political analysis; cannot reasonably clever politicians devise means to get around such an amendment? Currency inflation is certainly one such solution.

The authors make many excellent debaters' points, but their analysis is marred by flaws of omission and logic. Political scientists would probably find most serious their lack of attention to real-world political institutions and differences among democratic systems. They discuss "institutions of fiscal choice" only in a limited sense referring to methods of financing government. This is well done, in particular their account of the politics of monetary policy, but little mention is made of political parties, Congress, class conflict, or differences in interests or ideology among voters or politicians. The American federal system is relegated to a footnote (p. 58), where Buchanan and Wagner note that the states and cities have accounted for most of the growth in federal spending since 1960. Contrary to the book's major thesis, however, state officials have managed to raise taxes, and state budgetary limitations have not constrained growth in spending. Since these nonfederal units cannot create money, and face legal constraints on indebtedness, the authors blame the stimulus of federal spending. Yet has not the demand for federal revenue come from the states as much (or perhaps more) than from Washington? Could it be that popular support exists for an expanded government sector, even if increases in highly visible sales, proportional income, and property taxes are involved? The authors are unwilling to consider such alternative explanations because of their concern for deficits and government growth. Nevertheless, the Council of Economic Advisers supported a large federal deficit in the mid-1970s at least partially to offset the deflationary effects of state surpluses. Certainly a broader consideration of the politics of fiscal federalism is needed.

A second difficulty is the absence of quantitative analysis. The book's theoretical structure is buttressed by many numerical examples, but these often appear to have been chosen selectively. Thus the only table in the text (p. 111) purports to show a relationship between deficit spending and expansion in the money supply from 1947 to 1974. But the federal deficit is shown in current dollars; figures in constant dollars, or debt expressed as a proportion of GNP, would show a considerable *decline* in the federal deficit over this period. The recent increase could be attributable to OPEC, the balance of trade, and the 1974–75 recession as much as to the political application of Keynesian economics; but again, alternative explanations are given short shrift.

A third difficulty concerns the basic logic of their argument. Applied Keynesian economics may, under certain (non-Depression) conditions, produce inflation, soaring deficits, or a growth in government spending, but the relationships among these outcomes merit clarification before one can conclude that democracies inexorably tend toward deficits. Here, quantitative analysis or a comparative perspective might help. A comparison of democratic systems suggests many variations, even contradictions. Germany, for example, has very low inflation despite a public sector considerably larger than that of the U.S. The American states (with few exceptions, such as New York) have expanded spending without incurring deficits. U.S. federal spending has been quite stable in relation to GNP (19–21 percent over the 1955–1978 period, according to figures from the Congressional Budget Office and Brookings), although inflation has become a serious problem. Buchanan and Wagner (with John Burton) have written a parallel book, *The Consequences of Mr. Keynes* (London: Institute of Economic Affairs, 1978), on the British experience; a truly comparative treatment of the themes they raise in *Democracy in Deficit* would go far in clarifying their theory.

Persons of a less conservative bent than Buchanan and Wagner may disagree with their conclusions, but *Democracy in Deficit* is a well-written and thoughtful book, sure to provoke debate on a serious issue of policy and theory. Paying attention to politics and political institutions is a commendable trend in economics. Although political scientists can learn much from this discussion of Keynesian theory and current economic trends, *Democracy in Deficit* still falls short of a political theory of fiscal choice, especially when viewed in comparative perspective.

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From Contract to Community: Political Theory at the Crossroads. Edited by Fred R. Dallmayr. (New York: Marcel Dreker, 1978. Pp. xii + 172. $19.50.)

Fred Dallmayr contends that the articles here assembled are united in their concern with the conflict between individualism and community. In apparent counterpoint with Sir Henry Maine's celebrated "from status to contract," the editor implicitly characterizes his book as an exploration of means of bringing Maine's progression to its term; of finding devices for integrating the individuals who had been "liberated" by that progression into a genuine community.

An individualism-communitarianism dichotomy does play a role in most of the eight essays. Moreover, in the articles by C. B. Macpherson and Christian Bay it presents itself in something akin to the normative form just mentioned. Western societies are said to be afflicted by a fragmenting individualism destructive not only of community and its morality but also of individuality itself. In this circumstance a major task of the theorist is to identify a conception of society that transcends the atomistic egocentrism of "bourgeois liberalism" and "liberal democracy" while avoiding both the archaic, status-dominated notions of the pre-liberal period and the recent but barbaric formulations of totalitarianism.

In mentioning the ideologies discussed by Macpherson and Bay we in fact get somewhat closer to the common concern of the several contributors and to the substantial interest of the best of these essays. When Macpherson and Bay attack liberalism as the wellspring of present discontents, they implicitly present the rudiments of a conceptual scheme in which to identify and analyze the "individual," the "society," and hence the possible relationships between them. The schema advanced is dominated by ideological or—more broadly—ideational categories such as "individualism," "liberalism," "communitarianism," and the like, that is, by ideas in terms of which individuals and their interactions are identified and understood. Individuals and societies are not self-subsistent, invariant or even pellucid entities; nor can their character be ascertained by gathering "objective" information about material or other non- or extra-ideational realities. Individuals and societies, and hence relations among them, are what they are by virtue of the ideologies that prevail in the times and places in which they are encountered. As ideologies change, so do the individuals and their relationships.

The question implicit here, the question of how to conceptualize the individual and the society, is the explicit, self-consciously pursued concern of several of the essayists, most notably Jurgen Habermas and Robert Paul Wolff. Habermas, influenced by Marx, seeks a theory of social evolution which gives due weight to Marx's concept of the mode of production. But this concept is "not sufficiently abstract to encompass the universals of developmental levels" (p. 62) as they have in fact unfolded in human history. Attention must be given to "learning processes" and the "symbolic structures" in which they take place. Habermas does allow that Marx's concept of the mode of production directs attention to these considerations. But he contends that its application is restricted to "the domain of technical knowledge," a domain which is "decisive for the growth of productive forces" (p. 55), but which tells only a small part of the story of social evolution and form. Human beings also learn in "the domain of moral-practical awareness that governs the development of structures of interaction." Rather than simply reflecting changes in the mode of production, these processes "unfold in accordance with their own internal logic" (p. 55).

To understand that logic, Habermas sets aside Marx and Marxist-sounding discussions of bourgeois ideology and looks to theories of kinship from social anthropology and especially to Piaget's theory of the developmental stages of moral consciousness. The resulting construct appears to me to be at a considerable remove from any recognizable version of the "historical materialism" that Habermas claims to be "reconstructing"; indeed, some will think that it moves so far as to have become a species of idealism. However that may be, it projects an account of the individual-society relationship which, while indeed emphasizing ideational factors, is vastly more complex than the picture presented by Bay and Macpherson. Political theory will move out of this among its crossroads only when it has achieved an encompassing historical, social and psychological theory, that is, a full-blown "critical" philosophy.

Karl-Otto Apel and Svetozar Stojanovic pursue related themes in different contexts, the former in respect to the possibility of intersubjective standing for moral judgments and the latter in a critique of Stalinism. As with Habermas' article, familiarity with the other writings of these authors will contribute importantly to understanding the essays here published. Finally, Robert Paul Wolff, in the most self-contained of these essays, considers the claim that societies can and should be held responsible for at least some of the injustices suffered by their members.

Granting that large numbers of white American males have committed unjust acts against blacks and women, what exactly does it mean to say that these injustices are "social" in character? Despite admitted philosophical sympathy for methodological individualism, Wolff draws on Durkheim for a view of the society-individual relationship strongly analogous to Habermas' doctrine concerning learning processes occurring in symbolic structures. As a consequence of such processes and structures, white males have adopted "beliefs and attitudes" according to which blacks and women constitute social groups with a "real, rather than [a] merely nominal experience" (p. 73). The unjust acts that result from these beliefs and attitudes are therefore social both in origin and effect. The social elements that produce them "may indeed be unreal in the Hegelian sense that they are irrational. But they are nevertheless quite actual, and positive social measures are required to eliminate them." Wolff concludes that "individualism is not a description of our society; it is a goal toward which we can strive" (p. 79). Readers are likely to share, about the argument of this and other papers, something like the residual ambivalence that this sentence seems to reflect on Wolff's part. Several of the essays nevertheless repay attention.

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The American as Anarchist: Reflections on Indigenous Radicalism. By David DeLeon. (Baltimore: Johns Hopkins University Press, 1979. Pp. xiii + 242. $14.00.)

The radicalism of the 1960s provoked a renewed concern for the American radical past. Within the past few years, an increasingly heavy stream of studies analyzing and dissecting the roots of American radicalism has appeared. While contemporary concerns often do and should encourage us to reexamine the past, there is a particular danger that present political needs will lead to distortion or alteration of that past. David DeLeon's book, while often illuminating and insightful, unfortunately does not escape this problem.

DeLeon has not written a history of anarchism, nor has he set out to do so. His book is a series of reflections, with the strengths and weaknesses of that genre. He argues that there are two varieties of American radicalism—an indigenous type with an anti-statist or anarchist orientation, and a statist, primarily imported brand largely identified with European Marxism. The former is allegedly far more culturally significant even though its extent and influence have been vastly underestimated. Thus, for DeLeon there is a widespread indigenous American radical tradition which can be used and built upon by contemporary radicals if they would only learn to distinguish it from its European competitor.

The factors in American life which DeLeon believes have made our society particularly open to anti-statism include the dominance of a Protestant tradition emphasizing personal conscience, the persistence of a frontier spirit inculcating a sense of self-reliance and constant change, and a capitalist economic system with its ideal an Adam Smithian market. These factors have produced a variety of indigenous radical traditions: liberalism, right libertarianism and left libertarianism.

Liberalism is the most moderate of the anti-authoritarian strains, arising from the American Revolution and our aversion to big government. DeLeon seems to suggest that liberalism's turn towards regulation in this century indicates its intellectual bankruptcy. He spends far more time discussing right libertarians—such as Emerson, Thoreau, and Benjamin Tucker—who insisted on a linkage between freedom and private property and such left libertarians as J. H. Noyes, Edward Bellamy and Emma Goldman who yearned for a communal anarchism.

After criticizing the statist radical doctrines of Daniel DeLeon, the Communist party and the Socialist party, DeLeon turns to the 1960s revival of radicalism and discerns elements in both the Students for a Democratic Society and the Young Americans for Freedom that hearken back, respectively, to left and right libertarianism.

The dangers of such a simplistic and unidimensional categorization of American intellectual history are readily apparent. Individuals and organizations are crammed into slots which they fit badly. To label George Wallace and Ronald Reagan representatives of the right libertarian tradition because they attack big government or pointy-headed Washington bureaucrats is to misunderstand seriously American political life and libertarianism. For DeLeon, however, American hostility to the state and government is essentially anarchistic, a sweeping generalization which ignores such other sources of anti-statism as contract theory. DeLeon to the contrary, contract theory does not self-evidently arise from the same sources or tradition as anarchism. Indeed, the European origins of liberalism—which DeLeon briefly acknowledges—are a primary example of how indebted "indigenous radicalism" is to imported doctrines. American anarchism itself was strongly influenced by continental writers and figures.

DeLeon is concerned not only with demonstrating the strength of native radicalism, but also he insists that it is left libertarianism which is more humane. He charges, for example, that hostility to the state, when not connected to a concern for the common good, is merely a "bourgeois ego trip" (p. 131). Many of the left libertarians DeLeon admires, such as Bellamy and Noyes, substituted hierarchical or authoritarian communal structures for state authority. He argues, however, that "the drugs, sex, literature, and dress of the left represented a more thoroughly humanist and utopian socialist vision than that of the right" (p. 130). While DeLeon prefers the cooperative to the competitive mode of anarchism, he forgets that American hostility to the state has come far more often from a selfish desire for self-aggrandizement and privacy than from a desire for a communal world. The latter vision has been articulated far more often by Marxists and other statists. Left anarchism has never created roots in America—numerous failed communities attest to that—but capitalism has been quite successful.

HARVEY KLEHR

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egotiations: Social-Psychological Perspectives. Edited by Daniel Druckman. (Beverly Hills, Calif.: Sage, 1977. Pp. 416. $25.00.)

he Negotiation Process: Theories and Applications. Edited by I. William Zartman. (Beverly Hills, Calif.: Sage, 1978. Pp. 240. $16.95, cloth; $6.95, paper.)

These books on negotiation reflect different onceptions of how to go about trying to nderstand human conduct. Daniel Druckman's pproach is that of experimental social psyhology, a discipline organized to discover mpirical regularities in the interactions of a ew individuals by exploring, through conrolled experiments, the relationship among a mall number of variables. Because negotiations re often small group affairs which take place in relatively structured environment (in contrast o, say, revolutions), the approach would seem o be relevant. But it also tends to become arrow and ritualistic. And the result of prorammatic rigor is usually not a new way of eeing things but rather the perpetuation of old ays. Druckman's book illustrates the concepion of science as the patient and optimistic ccumulation of empirical findings, and of heory as an ordered body of such findings. lthough interdisciplinary in the sense that its ontributors come from several fields, the ollection displays considerable unity of purose and method.

Thirteen chapters devoted to reviewing and ntegrating previous research on a number of opics are arranged by the editor in four ategories: (1) the character of negotiation onflicts, (2) the negotiation process, and in articular the relationship between strategies nd outcomes, (3) the effects of background onditions such as the personalities of the egotiators, and (4) the effects of factors rdinarily found in negotiations but often issing from the contrived situations of the ocial psychological experiment. The introducion attempts to suggest how the various influences" thus categorized might be woven nto an integrated theory. The result, however, s very far from being a theory of any sort. On he contrary, it is a jumble of considerations hat require rather than provide explanation. he individual chapters, while narrower in cope and thus more coherent, do not give the npression that much progress has been made n understanding negotiations as a result of ocial psychological research. Most of the new deas presented here seem to have had their rigin outside experimental social psychology, nd are novel only in the sense of having been mported from a larger intellectual world in which they have long been common coin. A good example is provided by several chapters in the first part which focus on the recent discovery by social psychologists that the conflicts in which negotiations occur cannot be understood wholly as conflicts of interest, but must also be understood as "*cognitive* conflicts or conflicts caused by different ideas, values, ideologies, and policies" (p. 106).

The final section, which attempts to come to terms with the complexity of "real world" settings, illustrates the opportunities missed by experimental social psychology as a consequence of its methods and its conception of scientific research. The chapters in this part concern the effects of factors such as conference size and mediation on the negotiation process and its outcome. We learn that an increase in the number of parties creates difficulties for negotiation strategy, makes it harder to reach agreement, and leads negotiators to seek ways of simplifying the issues. Mediation on the other hand can help bring the parties together. This is a slim harvest. These chapters do not begin to tap the richness of even those sorts of negotiations, in labor relations and international diplomacy, that have been closely studied for years. And no attention whatsoever is paid to negotiation in such contexts as legislation or litigation, although the phenomena of legislative agenda battles, plea bargaining, or jury decision making are no less worthy of study than those of international or labor relations.

Of course there is much useful and interesting material in these chapters that is necessarily overlooked in a review of the "skim and scold" variety. My suggestion is that there might be more such material if the experimentalists could overcome the self-imposed limitations of their craft, and especially their tendency to dismiss with disdain mere "case studies" and the "speculations" of the "wisdom literature." Such attitudes have done much to keep the social psychological tradition in a groove along which the present volume moves negotiation research hardly more than a few steps.

In contrast with Druckman's approach, which is "cumulative," that of William Zartman's collection purports to be "dialectical." Zartman has set out to create a "confrontation of competitive paradigms" (p. 9) by soliciting contributions from a sociologist, an economist, a social psychologist, and several political scientists. The second half of the book contains studies of the diplomacy of the nuclear test ban negotiations, the Vietnam peace talks, and several other international negotiations. All of this makes for more interesting reading, although whether Zartman has started a fight or merely orchestrated a chorus is debatable. On the one hand, the reader is indeed treated to a wider variety of theories and methodologies. Several of the essays pay particular attention to Zartman's own interesting conception of negotiation as a two-stage process in which bargainers seek to arrive at a generally acceptable formula before hammering out the details, in contrast to the usual assumption that negotiation involves a uniform process of concession. Zartman also causes us to pay more attention to negotiations as enduring relationships rather than temporary transactions. On the other hand, the Zartman book is actually narrower than Druckman's because the studies in it are largely focused on the relationship of strategies to outcomes, whereas Druckman and his collaborators cover a broader range of questions.

Both books tend to proceed on the assumption that negotiations in which rights as well as interests are at stake are not essentially different from those concerned with purely economic issues. And the studies in the Zartman volume, although free of many of the idiosyncrasies of the social psychological tradition, remain committed to the search for empirical generalizations that are valid under many different circumstances. Negotiation research guided by this conception of science attempts to push to a new level of understanding by generalizing across institutional settings. But it is possible that such research is at the same time obscuring important distinctions between different kinds of relationships that are being indiscriminately lumped together under the label of "negotiation."

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Public and Private Morality. Edited by Stuart Hampshire. (New York and London: Cambridge University Press, 1978. Pp. x + 143. $15.95, cloth; $3.95, paper.)

This is a collection of essays by five distinguished British and American philosophers: Stuart Hampshire, Bernard Williams, Thomas Nagel, Thomas Scanlon and Ronald Dworkin. If there were a truth-in-labeling law for books, Cambridge University Press would be liable for prosecution. The dust jacket and the foreword encourage the expectation that the essays will address "the problem of 'dirth hands' in politics . . . posed most strikingly by Machiavelli." In fact, only three of the six essays in the book address that problem at all directly. The other three treat issues which will be familiar to readers of books such as Rawls' *A Theory of Justice* and Nozick's *Anarchy, State and Utopia,* but which Machiavelli apparently never dreamed of and probably wouldn't have cared about if he had.

Of the three "dirty hands" essays, Nagel's deals with the version of the problem likeliest to be in the minds of the *Review*'s readers. That is, he tries to explain what it is about public roles that prompts us to accept conduct by public officials that we would regard as morally outrageous if engaged in by private individuals. Nagel does not deny that public officials may (indeed sometimes must) do things that are not required of or even permitted to private individuals. But he argues that a correct understanding of the discontinuity between the public and private spheres does not justify the view that a public role frees its occupant of moral restraints to the extent that many have supposed.

The other two "dirty hands" essays are further removed from the issues that one normally associates with Machiavelli. Williams asks, given that morally discreditable conduct is an inherent feature of everyday political life, how readily disposed to such conduct do we want our politicians to be? And answers: not too readily disposed, since "only those who are reluctant or disinclined to do the morally disagreeable when it is really necessary have much chance of not doing it when it is not necessary" (p. 64). It is not clear why philosophical analysis was required to reach that conclusion. In "Public and Private Morality," Hampshire contends that "the moral infamies of the American intervention in Vietnam" (p. 51) resulted from our policy makers having been captured by an over-simple model of rationality according to which actions are rational only if the reasons for them can be made explicit, where only consequences count as reasons. In this and another essay in the book, Hampshire flails away at consequentialism in moral thinking from the vantage point of an intuitionist position which recognizes a plurality of absolute (and possibly conflicting) moral principles not derivable from any single overarching principle to the effect that conduct is morally justified as long as it has certain consequences. Neither of Hampshire's contributions to the collection told me as much about the role of consequences in justifying the actions of public officials as Nagel's did with much greater economy and clarity.

Of the essays by Dworkin and Scanlon, readers of the *Review* will probably find the former of greater interest. Against the widely held view that the term "liberalism" no longer identifies "an authentic and coherent political morality" (p. 115), Dworkin argues, in a clear, persuasive essay, that if one understands the distinctive ingredient of liberalism to be a certain conception of equality, a case can be made that liberalism is not merely an unstable, middle-of-the-road compromise between more robust conservative and radical positions on either side, but a distinct, self-contained political philosophy in its own right. Perhaps there are some, but I do not know of another brief defense of that view as nearly successful as Dworkin's. Scanlon's essay begins from the observation that classical utilitarianism has been embarrassed by values such as equality, fairness and individual rights, and argues that a more adequate account of those values can be given within a theory that avoids the embarrassment but is, like utilitarianism, still consequentialist.

The best of these essays (to my taste, Nagel's, Scanlon's and Dworkin's) repay careful reading by anyone who does not identify political philosophy with Straussian divination. I do not believe that the remainder will be particularly informative for anyone with even a passing acquaintance with recent Anglo-American moral and social philosophy.

LEON GALIS

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Lenin's Political Thought: Vol. 1, **Theory and Practice in the Democratic Revolution.** By Neil Harding. (New York: St. Martin's Press, 1978. Pp. 360. $25.00.)

In the study of Bolshevik ideology one question that has faced everyone is the one concerning the relation of Lenin's ideas to original Marxism as well as to the Russian revolutionary traditions of the nineteenth century. Neil Harding seeks to show that it is a mistake to derive Lenin's political thought from *Narodnichestvo* or Jacobinism. Instead, he seeks to show Lenin to have been the most consistent spokesman of a Marxist orthodoxy adapted to Russian conditions by Plekhanov, and accepted as valid by Kautsky. He credits Lenin with having complemented and justified Plekhanov's political orthodoxies by thorough and original economic analyses which proved conclusively that Marxism indeed was applicable in Russia.

Harding convincingly brings out the peculiar features of Russian Marxist orthodoxy and shows Plekhanov as a genuine pioneer of Leninism, who later recoiled from what he had begotten; he is less convincing in his refusal to acknowledge any of these peculiarities as derived from the *narodnik* heritage. He makes a contribution also by stressing the economic arguments Lenin developed in supporting the political positions he took, though he cannot in fact prove that the latter inevitably were deduced from the former; instead, the economic analyses may often have been rationalizations of political positions taken beforehand. He also provides confirmation of the intensity with which Lenin felt himself to be a modernizer in a retarded society; this is brought out particularly by Lenin's dismissal of *narodnik* theories as *kustarnichestvo,* i.e., the outlook of the pre-capitalist craftsman. It is a pity that Harding links Lenin's own views only to the production methods of the machine shop and not to the managerial rationality pervading all complex organization. Having missed this Weberian glimpse at Lenin, he does not deal at all with Lenin's greatest contribution to revolutionary Marxism, which is his attempt to carry out the revolution with the methods of rational bureaucratic management.

In his effort to prove his thesis, Harding ignores or slides over much evidence to the contrary. For instances, he dismisses all information which points to the lateness of Lenin's conversion to Marxism, and pays no attention whatever to discussions of his personality. Weaknesses in Lenin's own arguments are not stressed sufficiently. For instance, in his attempt to prove that Russia had already embarked on the road to capitalism, Lenin adduced the growth of usury, interest bondage, commodity production, and related *pre-capitalist* formations, and then mechanically transferred Marx's developmental scheme to Russia—all this, as Harding rightly points out, in tune with the views of Plekhanov, Struve, and others. Lenin concludes from all this that the peasantry was breaking down into two classes—rural bourgeoisie and rural proletariat. Harding neither stresses the foolishness of these views, in the light of which Vorontsov and other *Narodniks* appear to have been vindicated, nor does he point out that after 1905 Lenin changed his tune, treating the peasantry as a class in its own right, with its own interests. Indeed, Harding's failure to recognize Lenin's post-1907 policy toward the peasantry as a fundamental change, involving the abandonment of his previous economic analysis and political position, appears to be the principal failing of this book—this and the absence of any treatment of the heated discussions which Lenin's new policy engendered within the RS-DRP. Altogether, after stressing the orthodox heritage within which Lenin developed his views, Harding does not sufficiently acknowledge the many retreats from orthodoxy which he staged.

In general, the treatment is not critical enough; and although Harding seeks to dissociate himself from the worst inanities of Soviet hagiographers, the tone of his book approaches theirs. Lenin is treated with admiration and sympathy as one who was right most of the time. His antagonists are described with the same invective that Lenin used, or at least with condescension. The occasional mild criticism of Lenin is voiced so timidly that it has a slightly dishonest tone. Harding is lamely apologetic about Lenin's frequent crudeness, rashness, and misjudgments. In discussing *Materialism and Empirio-Criticism*, that exercise in philosophic philistinism, Harding timidly acknowledges the purely political motives for writing this book and appears aware of its philosophic inadequacies, but glides over them. In many other cases, he mentions contradictions, simplifications, and other failings, but never develops them. In the end, Lenin always seems right, and his opponents puny. His total ideology is given a coherence and sophisticated structure which in fact it did not possess.

While Lenin's rivals are diminished in stature, Harding pits his own interpretation of this material against antagonists who inevitably remain unnamed and uncited. Often the views criticized are caricatures of what previous scholars have written. Many important works dealing with Lenin do not even appear in the bibliography and seem not to have been consulted, including relevant works by Dan, Haimson, Meyer, Arthur Rosenberg, and Aleksinski, to name only a few. Kautsky's *Agrarfrage*, which had a major influence on Lenin, seems not to have been read either.

Many key words in this book should have been defined with some care, including "democracy," "democratic centralism," and the philosophic terms used in *Materialism and Empirio-Criticism*. Harding's phrase, "a straight dialectical either/or" (p. 280) and other passages suggest to me he needs a course in the Marxian dialectic.

The English is atrocious. The editing is sloppy. There are numerous typographical errors, especially in the transliteration of Russian names and titles. In his own translations, Harding obscures the vital distinction between *robachie* and *trudiashchiesia*, i.e., between industrial proletarians and the broad masses of all those who work with their hands. Not bringing out the difference means obscuring the vital issue, for Lenin, of the relationship between these two social classes in the democratic revolution. It symbolized the inadequacy of this work.

ALFRED G. MEYER

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The Political Theory of John Taylor of Caroline. By C. William Hill, Jr. (Cranbury, N.J.: Associated University Presses, 1977. Pp. 343. $18.50.)

John Taylor of Caroline County, Virginia, was born in 1753 and died in 1824. He wrote five books and twelve pamphlets, delivered numerous legislative speeches at both the national and the state level, and left an extensive political correspondence. He was engaged in more crucial arguments over a longer period of time than anyone else in early American politics, yet his work has failed to receive careful and thorough attention.

William Hill has attempted to remedy this defect by writing the first comprehensive treatment of the political theory of John Taylor of Caroline. He rejects previous commentaries which cast Taylor either as an apologist for a particular issue, such as agrarianism or states' rights, or as a bridge to important thinkers, such as Thomas Jefferson or John Calhoun. He completely demolishes the Beardian economic determinism approach by carefully examining Taylor's formative years, and he severs Taylor from his contemporaries and successors by demonstrating how far their views diverged. Hill's Taylor is neither an ideologist nor a supporting actor but a political theorist in his own right.

A major political theorist, according to Hill, must have "a comprehensive system of thought that cover[s] a wide range of topics in an integrated fashion" (p. 282). Hill's thesis is that Taylor's concept of "the division of power" is the "unifying principle," or "central doctrine," which makes the wide range of his works internally consistent and systematically presentable. He argues that Taylor's views on such apparently varied and unrelated topics as political parties, aristocracy, separation of powers, federalism, disunion, and agriculture are actually united and integrated because they are applications of the doctrine of the division of power.

Hill is correct; there is a remarkable unity and coherence to Taylor's work. Unfortunately, Hill is more concerned with testing Taylor by the abstract standards of contemporary political theory than he is with taking the conemporary reader into Taylor's world and pro-iding a critical guide to the issues which oncerned Taylor. Thus we learn that with espect to political parties Taylor was a weak olitical theorist because he changed his mind oncerning their role in popular government. With respect to all the other issues, however, Taylor was a major political theorist because *we* an find the concept of the division of powers resent in Taylor's treatment of these issues, nd therefore *we* can declare Taylor to be onsistent.

Hill's Taylor is an important political the-orist because a unifying concept can be found n his varied works and he applied this concept onsistently. But what is important about the ivision of power concept? Was Taylor the first r the best interpreter of this concept, and how oes it differ from other competing concepts? On these, and other important theoretical uestions, Hill's treatment is sketchy and inade-uate. Taylor's central concern was to demon-strate that republican liberty was in constant danger and that the only solution was a firm attachment to moral principles. The "division of power" was a moral principle which Taylor introduced in 1814, and developed in the 1820s, to counteract the persistent threat of political and commercial despotism engineered by a privileged minority. Taylor is important because he presents a teaching on republicanism which differs from that of *The Federalist* and John Adams. His intellectual and social origins are within the Antifederalist political tradition.

There are several irritating qualities to the book. Despite the fact that all of Taylor's leading works have been reprinted within the last ten years, Hill persists in quoting from out-of-print Taylor source materials which have a different pagination from the more accessible editions. Is it "Lockian" or "Lockean," "state rights" or "states' rights," "division of power" or "division of powers," "president" or "President"? There are also at least 40 typographical errors. Nevertheless, Hill's work on Taylor is important because it is the first comprehensive account of Taylor's thought.

GORDON LLOYD

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How to Think About the American Revolution: A Bicentennial Celebration. By Harry V. Jaffa. (Durham, N.C.: Carolina Academic Press, 1978. Pp. xii + 183. $12.75, cloth; $5.00, paper.)

How To Think About the American Revolution is a "milestone" in Jaffa's attempt "to understand the American political tradition as it has understood itself" (p. ix), and worthwhile reading for those who pursue that goal.

The introduction (the first of five loosely connected essays) contends that the principles of the American Revolution are hardly understood today, citing as an example Hofstadter's *The American Political Tradition.* The second essay criticizes Willmore Kendall's thesis that Lincoln's emphasis on equality in the Declaration of Independence "de-railed" the American political tradition. Jaffa rightly criticizes Kendall for trying to read Locke out of the American political tradition, and for ignoring the difference between the Declaration's equality principle and more radical notions of equality. Jaffa makes many of the same points in the fourth essay, a response to M. E. Bradford's criticism of the earlier essay. He defends the connection of equality and justice, relying especially on Aristotle's (somewhat equivocal) concept of "proportionate equality."

The third essay, the central and most important part, is a sharp critique of Bicentennial lectures by Irving Kristol and Martin Diamond. Kristol is criticized for playing down the radical character of the Revolution and overemphasizing its "success" and the inarticulateness of the American political tradition thereafter. Diamond is taken to task for arguing that "the Declaration ... offers no guidance for the construction of free government," since there is nothing in it "which goes beyond the sentiment of liberty" (p. 76).

Jaffa's analysis of Kristol and Diamond provides a useful antidote to their (largely rhetorical) excesses. Kristol does omit the Civil War and its relation to the "successful" revolution. Diamond does argue that the Declaration is neutral on forms of government, although the Declaration's "sentiment of liberty" clearly precludes despotism and would sit rather uneasily with non-constitutional monarchy or aristocracy.

On the other hand, Jaffa consistently and improperly relegates the post-Declaration problems (and thus the Constitution, the *Federalist,* and such questions as "republican virtue") to a secondary place. This is partly because he strains to see so much in the Declaration. He argues, for example, that the Declaration of Independence demands democratic government, relying on Jefferson's argument *elsewhere* that there is a natural right to consent to taxation. He also sees the principle of constitutionalism in the "clear distinction" between consent to the form of government (among the self-evident truths) and consent within the operation of government (to taxes, in the listing of British abuses). At best, this distinction seems none too clear in the Declaration. Apparently Jefferson did not see the implications of his clear distinction as clearly as Madison did (or as Jaffa does), since he was rightly criticized by Madison for advocating too-frequent constitutional revision. Jaffa can only weakly account for such criticism by saying that it is "founded in Jefferson's own thought." But that raises the question of whether the Declaration leaves more unresolved than Jaffa admits.

Jaffa's "natural theology" of the Declaration involves a Creator who is the source of natural rights (and corresponds to the people acting in its constitutional capacity), and a three-personed God who is legislator, judge, and executive (and corresponds to the people acting in its governmental capacity). However interesting on its own merits, this is a reading into the text rather than a reading of it. Neither it nor vague references to legislatures and judges justify Jaffa's assertion that separation of powers is a part of the Declaration's teaching.

The fifth essay, criticizing the establishment of the APSA Leo Strauss Award for the best dissertation in political philosophy, seems detached from the rest of the book. Yet it does help to explain the otherwise puzzling fact that Jaffa, a devoted student of Strauss (a critic of modernity), is an uncompromising, sometimes virulent defender of one form of modernity, the Lockean American polity. The explanation appears in his remark that Strauss' *practical* teaching took the form of "a celebration of the virtues, above all, of Anglo-American constitutionalism at its best" (p. 171). Thus, Jaffa's book can be understood as a celebration of what he regards as the best form (practically) of a defective politics (theoretically).

In light of that purpose, one understands why Jaffa adopts a "deliberately polemical" style (p. 9). On the other hand, one can ask whether such polemics (often insightful, but frequently unfair) are the appropriate manner in which to deal with scholars like Kendall, Diamond, and Kristol who pursue his own enterprise (albeit in a different way) of celebrating the virtues of Anglo-American constitutionalism at its best.

CHRISTOPHER WOLFE

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Ideology and Myth in American Politics: A Critique of a National Political Mind. By H Mark Roelofs. (Boston and Toronto: Little Brown, 1976. Pp. vii + 262. $5.95, paper.

Mark Roelofs' book offers the reader a " 'general theory' of American government" (p 7) that is still attentive to "the particularity of the American experience" (p. 244). Its intellectual roots are in "a debate that developed . . . in the field of American Studies in the 1950s" (p. 243), yet it recognizes the need for and tries to work out "a methodology which will unite ideology and behavior as jointly constituting the world of politics" (p. 249). The broad scope of the book's ambition is highly commendable, particularly in light of the narrow specialization, with little effort at synthesis and interpretation, still characteristic of writing by students of American politics and government. Occasional lapses in awareness of related specialized scholarship are surely pardonable, and need not detract from Roelofs' accomplishment. (One example only: Roelofs refers to Locke's "defense of the English constitution yielded by the Revolution of 1688" (p. 89) despite his reliance on Peter Laslett's edition of Locke's *Two Treatises*, in which it is convincingly shown that Locke wrote well before 1688.) His central preoccupation is not with novel or esoteric materials but with "the plain and obvious" (p. v): major historical events, documents, figures, problems. "We all know what the evidence is; the question is, what now, in the 1970's, are we to make of it?" (p. vi).

What Roelofs' book makes of the evidence is most depressing. His lead question is, "Why has America failed itself, so often, so disastrously?" (p. 254). And his answer is "that in America the discrepancy, the tension, between myth and ideology is extreme, so extreme as to be self-crippling" (p. 4). "Myth" and "ideology" are stipulated in somewhat unusual ways. Both terms refer to shared frameworks of political consciousness, with "myth" used to distinguish national identification and aspiration at a fairly lofty level—"a people's legend and its hope" (p. 4)—and "ideology" reserved for more narrowly practical patterns of operative behavior. America's myth, according to Roelofs, is Protestant, its ideology bourgeois; the myth is humanistic, egalitarian, and generally virtuous while the ideology is avaricious transactionism and rather sordid. Each is characterized as "a remarkably self-consistent whole" (pp. 38, 42), mainly because "for purposes of analysis we have separated ideology from all the mythic elements of the American political mind" (p. 38).

The book uses the tension between what America aspires to be and what it is and does to analyze a great range of topics, often with insight and interest: the Constitution, the Civil War, electoral politics, alienation, the "great" presidents, Watergate and the impeachment process, and much more. Its conclusions are almost invariably critical. So it bears stressing that the distressing picture of American political life is not the artifact of some "ought" imposed by Roelofs; rather, "the American political system is a failure *in its own terms.* We are not what we want ourselves to be" (p. 240, emphasis added). But not only is the past distressing; the prospect is more of the same. The prospects of or for revolution are nil, and so is the chance for change within the system. Roelofs' only advice is to have "a period of sober reflection" (p. 242). But he has already argued that the interplay between myth and ideology creates a cyclical pattern of despair, violence, and return to tense normality that is remarkably stable.

In analytic terms, Roelofs' conclusion appears to be that our national political mind is so sick, so stuck in repetition-compulsion, and so paranoid that the national political equivalent of (psychological, not economic) depression would be a step towards health. One very much hopes that the brave bleakness offered by Roelofs and his owl is a product of the reactionary 1970s. But one wonders.

JOHN R. CHAMPLIN

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The Foundations of Modern Political Thought: Vol. 1, **The Renaissance;** Vol. 2, **The Age of Reformation.** By Quentin Skinner. (Cambridge: Cambridge University Press, 1978. Pp. xxiv + 305; vi + 405. $29.50, cloth; $9.50, paper, each.)

Underlying this magisterial survey of the transition from medieval to modern political thought is the provocative claim that the history of political philosophy can be successfully rewritten as "the history of ideologies" (p. xi). Because Skinner is exquisitely scrupulous in rendering the views of so many diverse thinkers (including Dante, Marsiglio of Padua, Machiavelli, Guicciardini, Erasmus, More, Luther, Calvin, Vitoria, Suarez, Beza, Hotman, Mornay and Bodin), his concept of "ideology" accumulates a wealth of different connotations. To appreciate the truly significant achievement of these volumes, in fact, it is important to distinguish between at least two separate ways in which Skinner actually uses the term: ideology as the general *mentalité* of a culture or age, and ideology as skillful (sometimes quite cynical) propaganda.

As is well known, Skinner advocates a "contextualist" approach to the study of the history of ideas in contrast to context-indifferent "textualism." But "contextualism" too is a polysignificant word, and (as it happens) there is an implicit reference to *two distinct contexts* attached to Skinner's double concept of ideology.

On the one hand, there is the background intellectual context, for example, the "normative vocabulary" or "the prevailing assumptions and conventions of political debate" reigning in early modern Europe (p. xiii). Although he sometimes claims to be interested in routinized or standardized patterns of thought for their intrinsic historical importance (p. xi), Skinner elsewhere admits that, so far as his present work is concerned, the "main reason" (p. xiii) for tracing out the *mentalité* or commonplace ideologies of, say, Machiavelli's and More's lesser predecessors is to provide a "benchmark" (pp. 180, 255) against which he can accurately gauge Machiavelli's or More's originality as political theorists. Stated in Skinner's own favorite Austinian terms, placing a work within this sort of context allows us to appreciate its "illocutionary force," since we cannot understand the precise impact of what someone is saying until we know what he is denying. The thrust or explosiveness of an utterance cannot be discerned acontextually. This is true since what makes an utterance so surprising is the fact that it was spoken or written against the odds, that its context made it such an unlikely or improbable innovation. By leading us on a long tour through a multitude of fourteenth- and fifteenth-century homilies about the necessity for princes to be loved rather than feared, always faithfully to keep their word, to combine *virtù* with the Christian virtues and so forth, Skinner prepares us to comprehend the power of Machiavelli's denial of these conventional pieties. By first setting forth the typical humanist tenet that a virtuous commonwealth must include "degree, priority and place," he can subsequently convey the extraordinarily radical nature of More's attack on social inequality and private property. It is worth stressing that Skinner's "benchmark contextualism" (my phrase) has an extremely orthodox end in view. It allows the historian "to return to the classic texts themselves with a clearer prospect of understanding them" (p. xiii).

Alongside this notion of ideology as the conventional intellectual milieu environing a "classic text," Skinner introduces the quite separate concept of ideology as deliberate political propaganda. This second sort of ideology, as already mentioned, corresponds to a second kind of milieu within which Skinner wants to situate theories: the political context within which doctrines are "applied" in order to "legitimate" certain courses of action. Skinner is addressing himself here, as he has done elsewhere, to Lewis Namier's tough-minded suggestion that political historians can safely neglect the history of ideas, since political principles are largely "flapdoodle" or *post festum* rationalizations for actions which people would perform in any case and for extra-theoretical reasons. Skinner's counterargument is simple and convincing. Most political agents need cooperation in order to reach the goals they set themselves. To mobilize others into cooperating, they must present their own goals in a way that makes them appear compatible with the goals of partners being wooed. But once agents "legitimate" their behavior by describing it in widely accepted evaluative terms (once German princes describe their expropriation of monastic lands as part of the evangelical Reformation, thus mobilizing support for their initially greedy cause), they are no longer sovereignly free agents, but are instead bound at least *to appear* to act consistently with the terms of their "legitimating ideology," lest they lose the support and cooperation they need.

Now, what makes this book a brilliant advance on (rather than merely an application of) Skinner's previous methodological writings is his success at simultaneously employing these two distinct forms of contextualism, each affiliated with a special use of the term "ideology."

There is a closely related methodological premise implicit in the major historical claims Skinner puts forward. Each volume is organized as a series of polemical refutations of misleading contextualist accounts of Renaissance and Reformation political thought. One of the main targets in the first volume, for instance, is Hans Baron's *Crisis of the Early Italian Renaissance.* According to Baron (and this is a thesis which Pocock too accepts), there exists a sharp "cleavage" between the traditional convictions of Italian political theorists prior to the fifteenth century and the new outlook attained by the Florentine "civic humanists" at the outset of the *quattrocento.* The rupture is said to have occurred in 1402, under the impact of the threat to Florentine liberties posed by the expansionist Milanese tyrant, Giangaleazzo Visconti. As Skinner shows, however, a strong republican or civic ideology can be discovered in Italy as early as the 1260s. It is embodied in an illustrious tradition, including the works of Brunetto Latini, Mussato, Marsiglio of Padua, Bartolus and Ptolemy of Lucca. Thus, says Skinner, it is misleading of Baron and Pocock to suggest that republicanism "began" in the immediate political context of threatened Florentine liberties.

Analogously, one of the chief targets of the second volume is Michael Walzer's *The Revolution of the Saints,* where it is claimed that there is something radically innovative and unprecedented about the revolutionary ideology of sixteenth-century Calvinism. Here again, Skinner looks backwards and finds that the purportedly novel "Calvinist revolutionary ideology" is actually composed of theoretical components which were developed prior to the upsurge of Calvinism, to some extent by Luther, but even more importantly, by the Catholic advocates of conciliar rights against papal "absolutism."

On the face of it, these two revisionist claims seem like products of traditional philology, of "influence studies" or "source exhumation." It is an age-old problem in geography: where does the river really begin? And Skinner has shown that Baron and Walzer misidentify the "origins" of the theories with which they are concerned. But to interpret Skinner's arguments as a mere expression of bibliographical curiosity (who said it first?) is to miss his underlying methodological point. The prime duty of a contextualist, Skinner obviously believes, is to avoid the vulgar assumption that there is only *one* (homogeneous) context within which political theories must be "situated." By focusing exclusively on the political and military context of 1402, Baron neglected the crucial *intellectual context* furnished by the republicanism of Bartolus, Marsiglio of Padua and others. By concentrating on the political and religious context of the Puritan Reformation, Walzer slighted the specific *intellectual context* of anti-absolutist arguments developed by the Catholic conciliarists. The theoretical point is obvious: contextualism must not be confused with reductionism of the Marxist or Namierite sort.

Following the tradition of Figgis, Mesnard and others, Skinner wants to view the entire development of early modern (especially sixteenth-century) political thought as the gradual unfolding of the modern concept of the state. Thus, woven through the first volume we find the story of how anti-imperial and anti-papal arguments contributed to the modern notion of a plurality of territorially "sovereign" political units. To this Renaissance component of the modern concept of the state, Skinner adds (in the second volume) two typically Reformation components. He tells us first how the privatization of religion led to the idea (and the reality) of an increasing concentration of power in the hands of secular rulers, and then how the religious wars in France led to the *politique* assertion that the state no longer need furnish theoretical solutions to theological conflicts, but must be satisfied with providing a *modus vivendi* among discordant religious groups.

Skinner is obviously fascinated by the problem of "meaning change," and he devotes several pages to the standard topic of how the word "state" arose from its medieval ancestor *status.* Nonetheless, he is too good an historian to wrench his material into a preconceived pattern. Instead of aggressively stamping each part of his study with the fully articulated Weberian concept of the impersonal, bureaucratic state, Skinner tends to give a great deal of leeway to each thinker and problem-constellation. He never claims, for example, that Marsiglio of Padua was trying (but failing) to come up with Bodin's juristic theory of the modern state. More than in the Whiggish works of Figgis and Mesnard, as a consequence, there exists a slack relationship between the story of the emergence of the modern concept of the state and the account Skinner gives of the twistings and turnings of early modern political thought. Rather than being a defect, however, this is a virtue—another sign that methodological scruples pay off in the application.

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The Developmental Logic of Social Systems. By Henry Teune and Zdravko Mlinar. (Beverly Hills, Calif.: Sage Publications, 1978. Pp. 174. $14.00, cloth; $6.95, paper.)

The Social Ecology of Change: From Equilibrium to Development. Edited by Zdravko Mlinar and Henry Teune. (Beverly Hills, Calif.: Sage Publications, 1978. Pp. 296. $14.00.)

Zdravko Mlinar and Henry Teune have been the driving force behind the Research Committee for Social Ecology of the International Sociological Association. They have constructed an ambitious research program and have stimulated others to delve more deeply into the approach of social ecology and the study of development. These two volumes are part of the product of their labors.

In *The Developmental Logic of Social Systems,* Teune and Mlinar claim to address "the most perplexing theoretical problem in the social sciences: the explanation of social change." In fact, they deal only with developmental change, admitting (p. 45) that this "may only be a small domain of the social change taking place." Nevertheless, they do have interesting, closely argued and provocative ideas concerning developmental change. Their opening sections, detailing their general stance and systems-theoretic approach, are excellently constructed, luring the reader to read on with genuine interest.

By contrasting three kinds of systems (mechanical, ecological, and developmental), Teune and Mlinar call for an analysis of the latter in their own terms rather than in the imagery of mechanical or ecological systems. Developmental systems are defined by "their capacity to generate new properties and integrate them which results in changing the nature of the components and their relationship." Systems have components, subsystems, and levels. They can vary in their structural configurations and they have properties which are distributed across components. The major focus of Teune's and Mlinar's analysis is the set of components, never adequately delineated, and the changing distributions of properties across these components. The sharing and not sharing of properties by components is pivotal in their theory. Variety is the number of subsets of identical properties. Diversity is the extent to which the sets of properties of the components are not identical. Integration is the probability that a change in one component will lead to changes in other components. Finally, development of a system is its level of integrated diversity. It is a subtle and troublesome concept to deal with.

The dynamics of structural development are articulated in a variety of "laws." Given a count of components, properties, and their distribution, there is ample scope for simple combinatorial exercises. Many of the "laws" are the (tautological) outcomes of such exercises and, as such, do not constitute social laws at all. The use of their "laws" leads to statements like this one:

> The core of the structural dynamics of developmental change is that the greater the total number of properties available for distribution, the greater the variety of those properties; and the greater the number of properties distributed to each component, the greater the diversity of the total system. To the extent that this greater diversity is integrated, it will yield greater variety. The probability that diversity will become integrated increases to the extent that

> greater diversity is accompanied by greater similarity. And the more diversity there is, the more rapidly variety increases. An increase in one accelerates the other . . . and the integration of diversity increases. Thus, the level of development predicts the rate of development (p. 53).

Laid over this is a distinction between coaction systems (which are not developmental), interaction systems and transaction systems. Each behaves differently, each has different dominant activities and each has different developmental dynamics. Teune and Mlinar articulate a cross-level dynamic whereby variety, diversity and integration can be moved between levels.

The ultimate point of development appears to be transaction systems whose only components are individuals. Equality abounds. Indeed, "widespread cooperation would further the transformation of interaction systems into transaction ones where properties proper [skills, knowledge, values] are not scarce and are not located in unique places in time and space. Whatever remnants of competition and conflict [that] remain would recede into irrelevancy" (p. 171). This has all the hallmarks of a naive utopian vision—only there is a theory behind it. Whether one regards the theory as viable will depend on one's views concerning clarity of expression, relevant concepts, evidence and scholarship.

Power is ignored and the idea that different components may have different interests is not really dealt with. Resources are exogenous to the theory, as is control of resources and, implicitly, resource abundance is assumed for transaction systems. Underlying the analysis is the unrealistic assumption that developed social systems are free from ecological restraint. Technology, while seen as crucial to development, is very briefly considered and inadequately incorporated into the argument.

Evidence and scholarship are virtually absent from this volume, a consequence of the authors' decision to write a theoretical treatise exclusively in terms of "their" ideas. They are concerned with testability, correctly observing that "testability is a question of measurement systems rather than the generality of the theory." But unless the components and properties are carefully specified (a mammoth and complex task), variety, diversity and integration can take any value. Teune and Mlinar sketch some of the characteristics of an appropriate measurement system but they do not provide such a system. The epilogue, claiming that they have (selectively) accumulated evidence in support of the theory, without revealing that evidence, provides cold comfort. Moreover, the fragments that are supported (e.g., "patterns of population concentration in countries becoming developed or industrialized") could be obtained from other theories.

The absence of scholarship is, if anything, more worrisome. Initially, Teune and Mlinar do assemble, rather persuasively, their ideas on the dynamics of development. In the latter part of their volume, they then deal with the production, consumption, and use of system properties, exchange, value systems and manifestations of developmental change (like urbanization). Here many of the ideas are readily apparent in many areas of social science literature and are simply being repackaged. Relevant evidence is ignored or only casually appealed to. While the authors legitimately dismiss much of the "development literature," their work suffers from their practice of ignoring the broader literature or surreptitiously smuggling fragments of it into their argument. To use their concepts as images, they ignore the variety and diversity of the literature and thereby fail to integrate it into a truly coherent theory. There is none of the richness and interpretive detail of, say, the theoretical argument of a work like Lenski's *Power and Privilege*. Instead, there is an underdeveloped and largely unsubstantiated thesis, which is unfortunate, given the rich potential of the authors' initial ideas.

The Social Ecology of Change issues a clarion call for the reorientation of human ecology and ecological thinking in the social sciences. Social ecology has been moribund for several decades and the editors counsel us to take the high road of examining social systems that develop and to deal with the spatial and temporal dynamics of development within a social ecological framework. The call should be heeded.

The book is divided into four sections: (1) Social Ecology at the Crossroads; (2) Socioeconomic Processes in Space and Time; (3) Human Action and Ecological Structure; and (4) Methodological Approaches to the Study of Socio-Ecological Change. It is the product of a workshop on human ecology held in Ljubljana, Yugoslavia, in 1976. Those papers most consistent with the organizers' objectives (see *The Developmental Logic of Developmental Systems*) were collected in this volume, together with an introduction and the distillation of some of the participants' deliberations. In the main, it is a useful collection.

Mlinar's thoughtful discussion of the transformation of social ecology provides a fine foundation for the volume. The editors' distillation is uneven as it is permeated with their ideas and other ideas that do not fit well with hem. Further, as a workshop participant I see hat, in the methodological segment, the ditorial work strips all of the references cited y the contributors and quotes published maerial without acknowledgement. At the very east, this is unhelpful to the reader; this hapter should not have been included.

The remaining articles are uneven in quality nd space limitations preclude individual examiation of them. However, I would particularly ecommend the papers by Mlinar, Trampuž and erligoj on the social ecology of developmental hange; by Janson on the factorial study of ocio-ecological change; and by Sicherl on the ynamic measurement of disparities. The workhop could have a dramatic impact upon social cology, but whether that impact will be within he conceptual framework advanced by Teune nd Mlinar remains to be seen.

PATRICK DOREIAN

niversity of Pittsburgh

sychoanalytic Politics: Freud's French Revolution. By Sherry Turkle. (New York: Basic Books, 1978. Pp. x + 278. $12.50.)

This is a first-rate book. Sherry Turkle andles a vast sweep of ideas with assurance. er reconstruction of the arcane thought of acques Lacan is fair and subtle and, above all, ucid. The book is fascinating to read, placing he theoretical ideas within the drama of their istorical and cultural setting.

Since the May 1968 student uprising, sychoanalysis has taken a major place in the heory and practice of the French Left—and in rench popular culture as well. How did this ome to pass in a society which was resolutely nti-psychoanalytic? How was a radical current n psychoanalysis discovered when the maintream of that discipline is so conservative?

These are some of the questions Turkle oses. In providing the answer, she gives us a anaoramic view of French society and intellecual life today and places them in historical ontext. Focusing more narrowly, Turkle reonstructs the role of psychoanalysts themelves, and of their ideas, during the May 1968 vents. She follows these beginnings through he subsequent development of a psychoanalyically influenced radical ideology. She traces he impact of this ideology in the university, he mental hospitals and the psychoanalytic ocieties, basing these institutional studies on nterviews which enliven the text. Enough etail is provided for us to grasp the labyrinhine quality of political life in these settings.

At the center of all of these developments stands the figure of Jacques Lacan, whose charismatic leadership, as well as his novel formulations, have given psychoanalysis such a prominent place on the French Left. American readers may have already encountered Lacan as a representative of French structuralism, as an influence on the Marxism of Louis Althusser, and as one of a number of major innovators in contemporary psychoanalytic thought.

Turkle's approach of examining the thought in the context of Lacan's life and times is particularly apt in this case. Lacan's ideas—and their influence—cannot be appreciated through the published text alone. His public life as a trainer of analysts, a founder of institutions, and a lecturer whose persona is itself part of his message must also be understood.

Turkle shows respect and understanding for Lacan's cryptic mode of communication through which he attempts to speak directly from and to the unconscious. Yet she speaks in her own straightforward and lucid fashion. The result is easily the best I have seen among a number of expositions of Lacan's ideas in English. Her epilogue, "Lacan in America," is a particularly helpful interpretation and completes some of the ideas presented in chapter 2.

Turkle sees Lacan's insights into the "presence of social forces within the individual" as having genuinely radical political import—as restoring to psychoanalysis its original significance as a "subversive science." When she sees the profound difficulties and paradoxes which have developed in the Lacanian movement, she interprets these as universal dilemmas for any psychoanalytic politics. Since she has joined the Lacanians in viewing American psychoanalysis as totally preoccupied with adaptation, she sees no alternatives from that quarter. (Nor does she make any mention of the British "object relations" school.)

It is my belief that there are profound new currents (and old ones being rediscovered) in psychoanalysis and humanistic psychology which escape some of the paradoxes inherent within Lacan's system. The political implications of these psychoanalytic views have not yet been fully articulated, but efforts in that direction are beginning to appear. These efforts involve an appreciation of: recent American work in ego psychology such as that of Mahler, Kohut, and Kernberg, or of Loevinger and Kohlberg; the work of the British "object relations" school; and the insights of Reich (and his student Lowen) on character analysis and muscular armoring. The combination of these psychoanalytic perspectives with insights from feminism and Marxist humanism offers the promise of a psychoanalytic radicalism that *is* a genuine alternative to that of Lacan.

WILLIAM R. CASPARY

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Commitment and Change: Georges Sorel and the Idea of Revolution. Essay and translations by Richard Vernon. (Toronto: University of Toronto Press, 1978. Pp. viii + 148. $15.00.)

Historians of political theory have not done well by Georges Sorel (1847–1922), who has been more talked about than read, labeled more often than understood. In part this springs from the bewildering range of political positions he espoused during his lifetime. Additionally, few of his writings have been easily available and, until quite recently, the only work available in English was his *Reflections on Violence*. Within the past few years, however, a serious attempt to understand Sorel in depth has begun to take hold. Richard Vernon's book is an important, if incomplete, addition to this literature. The main topics he discusses are Sorel's conception of revolutionary commitment and change, his reflections on the relationship between thought and action, his critique of Marx, the influence of Bergson on Sorel, and Sorel's location in the history of political thought.

This is, however, a curious book, exciting and disappointing at the same time. In large part, this is because of its form—a 70-page essay followed by 50 pages of extracts from Sorel's untranslated essays, prefaces, and books. Neither separately nor together, however, do they provide a thorough examination of Sorel's conception of revolution. Nevertheless, the essay is one of the most sophisticated and illuminating discussions of Sorel that has appeared in print, and it is a shame that Vernon did not see fit to develop it more fully. He suggests, for instance, that the basis of much of Sorel's thought is a form of conservatism, with emphasis on order, decentralization, and morality, an "ethics of association embodied in a concrete way of life" (p. 61), and it is this that provides an underlying unity to much of Sorel's thought. Yet the form and content of Sorel's morality is hardly explored; indeed, the terms Sorel uses—"the heroic," "the sublime," "regeneration"—are not discussed at all. To take another example, Vernon argues that Sorel's conception of the disjunction between knowing and acting is central to his understanding of the problematic nature of revolutionary commitment and change. Yet he never attempts to assess the validity of Sorel's contentions.

Sorel is important, Vernon thinks, for several reasons. He anticipates some of the most effective critiques of Marxism, his writings are a sustained meditation on the relationship between theory and practice, and he provides us with something that is missing from contemporary thought, "a chastened and sceptical view of revolution" (p. 71). Sorel, moreover, is a figure who has been much misunderstood, particularly by those who depict him as an "irrationalist." The term, Vernon argues, is ambiguous, and has foreclosed too many questions. Sorel was "much more than has generally been thought, a man of reflection, who denied, however, that the stable and unitary constructs of reflection had any secure place in the essentially multiple world of action" (p. 71). He emerges, too, in Vernon's account, as a "*clerc* of violence, preoccupied with the processes of contemporary change but denying that his own preoccupations could or should enter into the processes themselves" (p. 71). But Vernon is unwilling to leave it at that. If Sorel is to be located within the history of political thought, it can only be in a French tradition "with an essentially moral emphasis," one which is "acutely distrustful of the notion of sudden transformations leading to new and unknown orders" (p. 61). For Vernon, Proudhon was Sorel's precursor, Camus his successor.

The translations add little to the essay and are too brief to be helpful to anyone coming to Sorel for the first time. Indirectly, however, they point up the need for a complete critical edition of Sorel's works.

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American Politics

Television Network News: Issues in Content Research. Edited by William Adams and Fay Schreibman. (Washington, D.C.: School of Public and International Affairs, George Washington University, 1978. Pp. 231. $6.00, paper.)

Television Network News: Issues in Content Research, edited by William Adams and Fay Schreibman, takes one step toward informing political scientists of recent work about television news research and methodology. Five of the articles are written by political scientists, seven by communication specialists, two by historians, and one by an instructional media specialist. The essays reflect this diversity. The volume's explicit purpose is to "bring together into one forum discussion of some of the more important issues in the study of network news content" (p. 5).

The first section purports to discuss an overview of research about news. The initial essay summarizes studies about production, content, and effects research. The first involves work about the determinants of news content during production, while content research analyzes patterns of manifest content. Effects research investigates the social and political consequences of news. One may dispute some of the conclusions in this essay, such as the assertion that "agenda-setting has become the most theoretically-developed and empirically-tested distinct area of impact research" (p. 30), but Adams provides a credible and useful outline of an enormous body of interdisciplinary research.

Comstock and Cobbey argue that television has "heightened the urgency" of problems citizens face in evaluating news (p. 48). There are few concrete standards which the public can apply in evaluating the medium. Paletz and Pearson review six recent books about network and local news which concern either news organization, content, or impact. On the basis of a lucid, perceptive analysis of the studies, the authors argue that greater emphasis should be given to some largely ignored areas of research, including uses and gratifications associated with video news. Nowhere, however, is a rationale given for the selection of these particular works, all published between 1973 and 1976.

A second section concerns research methods and resources. Schreibman provides a particularly valuable survey of news collections housed in several television archives. Each is described in terms of the range and scope of news holdings, access, procedures of use, costs, and organizational goals. The holdings of the oldest and richest source, the Vanderbilt Television News Archive, which includes the Television News Index and Abstracts, provides an example of how each is treated. Also discussed are special services, modes of access, fees, videotape format, and organizational aspirations. Lichty and Bailey present "practical advice" about content analysis. Having assessed some "wrong reasons" for conducting content analysis, such as trying to infer "effects" rather than manifest content and proving accusations rather than testing hypotheses, the authors discuss concerns of sampling, defining units of analysis, forming categories, executing coding procedures, and analysis and presentation of data. Points are illustrated by examples from published literature. While the essay is informative and useful in developing studies, research scholars will find little in it that is new.

Culbert discusses the value of idiographic approaches in contrast to nomothetic approaches employed by political scientists interested in building models. Understanding visual images presented in television news, he argues, is particularly important to the "fullest possible comprehension" of past events (p. 140). However, Culbert's apparent denial of selectivity in his quest for "complete comprehension" of visual images of the Loan execution in the broader context of Vietnam is puzzling. How can knowledge be drawn from any set of events, visual or otherwise, without selectivity and generalization?

Deploring the tendency to ignore visual characteristics in television news research, Adams describes examples of how visual images have been included in content analysis, and how image content may be determined by events, production, and the interaction between visual and audio aspects of programming. He raises some particularly interesting questions about the isomorphism between film and text in news coverage.

The final section discusses future directions of news research. Patterson urges that content analysis become more theoretical and that greater attention be devoted to scientific adequacy. Attention should be paid not only to reliability and validity in coding but also to research designs which emphasize comparisons among different forms of media at different times.

Gerbner and Signorielli assert that their "new, realistic, and more appropriate framework" will improve research about television news (p. 189). They argue that research about news should employ content and impact categories which are suitable to explicitly entertainment programming.

We are, however, left wondering whether most analysts are not aware of many of the problems cited.

Robinson also argues for a new direction, if a "reactionary" one, in television research. He lays out four interpretations viewed as alternatives to Epstein's organizational theory of news. Having reviewed the "newsworthy" interpretation, the "reality" interpretation, and the "collage" interpretation, Robinson elaborates what he calls the "attitudinal-political" interpretation, which assumes that the political attitudes of newswriters and reporters influence news content. Robinson argues for the value of analyzing news about less structured, less clearly "political" events.

Although of interest to social scientists concerned with television news research, these essays have a protean character, not unlike the alleged character of individual viewers of network news. Little binds them together except a concern with TV news. The preface serves little purpose, and no introductory material ties the contributions together. Moreover, the essays are of uneven difficulty, scope, and sophistication; still, the volume should be read by students of television news.

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The William O. Douglas Inquiry into the State of Individual Freedom. Edited by Harry S. Ashmore. (Boulder, Colo.: Westview Press, 1979. Pp. xvii + 250. $15.00.)

This volume is the result of a project of the Center for the Study of Democratic Institutions to monitor concerns of individual freedom that marked the career of Justice William O. Douglas. It consists of 11 papers, ranging in length from 8 to 38 pages. Most, if not all, of the authors had served as law clerks to Justice Douglas.

Each contribution focuses on an aspect of one of four general organizing themes: (1) "Fundamental Freedoms: Areas of Human Behavior and Personality Immune from Governmental Interference"; (2) "Freedom and Law Enforcement"; (3) "Freedom and Governmental Expenditures to Advance the Common Good"; and (4) "Freedom and the Duty of Government to Control Private Power."

Each essay typically attempts to survey the present situation. Problems and dilemmas are posed. An occasional alarm is sounded. Questions are raised, and sometimes guidelines are provided for a preferred approach to the issue of freedom under scrutiny. Court cases, governmental actions, laws, and current bills comprise the principal sources for the studies. Developments into 1978 are covered.

The nature of the topics may be gathered by an example from each of the four theme areas. Leonard B. Boudin contributes a brief paper on "Freedom of Inquiry in the Sciences," dealing with the tensions between the principle of unrestricted scientific inquiry, on the one hand, and the realities, on the other, of governmental regulation and increasing apprehensions raised by genetic and other controversial research. Boudin argues that "while the First Amendment precludes regulation of research, it permits regulation of the technological aspects (including the methodology) of the research" (p. 40).

In a concise chapter, Jerome B. Falk, Jr. explores the erosion in the law of searches, dealing with such recent cases as that of the *Stanford Daily* (in which he was counsel) and such situations as the screening of airline passengers and prospective thefts of plutonium.

In a lengthier, well-organized paper, William A. Reppy, Jr. thoughtfully summarizes and analyzes problems and issues of governmental funding as they bear on academic freedom. And, as a final example, Robert B. McKay raises stimulating questions "in the search for a proper balance between professional freedom and public control" (p. 203) as he examines issues of ethics and the law, medicine, the military, accounting, and journalism.

Other chapters examine the tension between the right to privacy and the right to know, control of the intelligence agencies, private discrimination, access to the media, governmental investigations, protecting persons from themselves, and the rights of those working for governments.

As intended, the volume is an "inquiry," a present inventory of aspects of individual freedom in a rapidly changing society bound by the Bill of Rights. It is addressed to the lay reader. A major purpose is to further public discussion. Portions will date quickly because some of the writers deal with current matters such as bills in committees. Documentation of some papers is elaborate; for a few it is scant.

Much of the material is familiar to political scientists and fairly elementary. For a political scientist the book's usefulness lies mainly in the provocative questions raised, some of which suggest intriguing research possibilities.

HOLBERT N. CARROLL

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Leadership in America: Consensus, Corruption, and Charisma. Edited by Peter Dennis Bathory. (New York and London: Longman, 1978. Pp. vi + 200. $6.95, paper.)

This book draws on several independent studies. Bruce Miroff's chapter on JFK rests on a thesis; Dennis Hale's on Curley comes from a book on the Boston mayor. W. C. McWilliams' account of LBJ is the judgment of a mature political scientist conversant in detail with Johnson's politics. Rev. W. K. Smith is a thoughtful disciple of Martin Luther King, Jr. Yet while each contributor has his own bent, the book derives its unity from a meditation on Tocqueville. From him the authors have learned that one thing is needful: to create a form of political association that will educate the majority in the art of self-government. This task governs the choice of subjects for the nine essays in political biography that comprise the body of the volume. These amplify Bathory's contention that this task has been since 1896 the central problem of American politics. Tocqueville also provides a key to the book's thematic terms. Thus we may equate *corruption* with what he termed *individualism.* Bathory teaches that Mark Hanna's victory over Populism was the product of a comprehensive strategy to foster individualism. H. A. Plotkin argues that the managerial consultant P. Drucker has promoted individualism rather than Tocqueville's art of association, discouraging corporation managers from engaging in politics. By recovering Tocqueville's quest for associative forms to replace the New England township, the authors hope to bring political science back to Tocqueville's intention, to promote sober and reasonable deliberation in democratic politics.

One may doubt that the approach adopted is suited to this last task. For political science might well contribute more if its best recent efforts were properly interpreted. Bathory and his contributors fail to supply a comprehensive analysis of the predicament of our proper leaders, the duly elected ones. Such an account might start from recent controversies over reapportionment, the voting age, the electoral college, campaign financing, the rules governing party primaries, and the expansion of the judiciary's purview. What Tocqueville called the *forms* of American democracy, the laws and customs in and through which the majority governs, have been under attack since he wrote. The egalitarian and individualistic movement he warned of has swept away one after another of these forms, always in the name of democratic reform. The authors are aware of this, although they do not trace the history of the unforming of American democracy in detail. But they do not remark how the pace of demolition has increased during the last 30 years; nor do they draw the consequences for political leadership.

That one should may be suggested by a glance at Marc Landy's fine essay on George Meany. He writes with more sympathy than the other authors can muster for Madison's political science. By adopting Madison's standard, Landy defends Meany by our most respectable criteria; moreover, our elected representatives fail while Meany passes. Landy argues persuasively, in two notable cases, when Meany endorsed the admission of refugees from Vietnam, and when he crushed local union resistance to busing, that Meany was just what Madison sought by means of representative institutions: a voice bringing liberality, tolerance, impartiality and reason into public deliberations. Yet Landy never weighs the effect on representative government of the part of Meany's strategy for building a national union which Madison would have thought most problematic. The drive to unionize public employees, combined with the use of labor voting strength to punish Congress members who opposed it, is not assessed in its effect upon the impartiality or rationality of elected officials. This failure to carry through Madison's science of representative government may point to a larger problem.

Bathory begins by comparing Tocqueville with Madison, for whom political science was to introduce impartiality into democratic politics. Tocqueville rejected Madison because "he feared a world that had lost a sense of the possibility of strong commitment to anything. . ." (p. 19). Because commitment rather than impartiality is for Bathory and most of his contributors the crux of democratic civility, they depart from Tocqueville's reliance upon judicial forms, upon jury duty as a means of political education, and upon the legal profession, to introduce respect for law, sobriety, and impartiality into democratic politics. This departure may reflect a deeper reservation about Tocqueville. Bathory's objection is one that led Weber to make liberal principles the object of a leap of faith. But Tocqueville still knew science as the ascent to first principles, to "the light by which we are guided," and thought he was teaching the secure principles of republican government. If we could see our way clear to such principles, we could be liberals without blushing, without guilt, even without Rawls; but like the authors of this volume, we think we cannot. This is consequential, for principles are needed to bring liberal democracy to light as a whole one which we can pass judgment. Tocqueville had exhibited a distinctive regime in which the majority ruled through definite forms; what was missing in America but necessary to make democracy liberal and thus choiceworthy was supplied by him by deduction from prinicple. By contrast, *Leadership in America* is silent about liberal democracy as a whole. It confronts us with fractious parts, leaving us like delegates at a party convention to form them into a whole. If we strain our eyes, the most we might discern in these chapters is the possibility that with proper leaders the unions, cities, and a national party might perform Tocqueville's task of "transforming a faceless mass into a citizen-public" (p. 190). We may even glimpse a vision of how the diverse constituencies of the Democratic party might be formed into self-governing parts and combined into a working whole. But we cannot see the relation between this partisan whole and the liberal democratic whole which Tocqueville delineated. In their convention the Republicans too think that they are putting the country together.

Leadership in America seeks to bring political science and political leadership together in the quest for a democratic civility, robust enough to withstand the rough and tumble of American politics, yet noble and difficult enough to make liberal democracy choiceworthy once again. It is an exceptional introductory text; the authors convey their most serious reflections on what they regard as the most important issues, but with an experienced teacher's respect for what the newcomer can grasp. Without condescension or jargon, the book is dramatic, engaging, controversial and nicely brief. If it is widely taught, as it deserves to be, it should compel us to ask whether liberal democracy can indeed be choiceworthy, and whether we scholars can in good conscience assert our democratic liberalism. It compels me to ask because the authors do not, strange to say, although they appeal to Tocqueville, who taught us that we could.

ROBERT EDEN

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Government by Judiciary: The Transformation of the Fourteenth Amendment. By Raoul Berger. (Cambridge, Mass.: Harvard University Press, 1977. Pp. x + 483. $15.00.)

The thesis of this book is simple—under the guise of "interpretation" the Supreme Court has revised the original meaning of the Fourteenth Amendment, has substituted its will for law and thus has contributed to the destruction of constitutionalism and the rule of law in America. The meaning of the Fourteenth, which the Court has consistently misconstrued (and through which most modern constitutional adjudication flows), is not, to Raoul Berger, so difficult to discover. In fact, the intent behind the amendment was rather clear, even unequivocal: "The key" to the meaning of the constitutional text "is furnished by the immediately preceding Civil Rights Act of 1866, which, all agreed, it was the purpose of the amendment to embody and protect" (p. 20). Designed "to embody or incorporate the Civil Rights Act," the Fourteenth would secure that act against repeal by a subsequent Congress and make it safe from judicial repudiation.

But the Civil Rights Act of 1866 (and hence, in this view, the Fourteenth) had "patently limited objectives," among which were the right to inherit, to purchase and sell real and personal property, to make contracts, to sue and be sued and to give legal evidence (pp. 24–25). It did not reach nor desire to reach the protection of any political or social rights, and surely not the right to have all votes equally weighed or to attend integrated schools. Thus almost all of what passes these days as constitutional "interpretation" is nothing more than unlawful "amending" by the Court, the substitution of its views for that of the framers and of the sovereign popular will as expressed constitutionally—in brief, government by judiciary.

It would certainly not be fruitless to reexamine the historical data and review the accuracy of Berger's historical understanding. Yet the truly important issues raised by this book become clear if we grant, *arguendo*, the adequacy of the historical investigation and concede that the broad wording of the Fourteenth Amendment had as its direct and immediate intent simply the validation of that early Civil Rights Act. What, then, does it mean to hold that the life of constitutional law is neither logic nor experience but, rather, careful legal history? On one level it is probably beyond controversy that the framers of the Fourteenth did not narrowly "intend" the immediate desegregation of all public schooling, the ultimate destruction of all malapportioned legislatures or the full application of the Bill of Rights to the states when they wrote the amendment (any more than the Founders, when they gave broad powers and ample means to Congress were in favor of establishing a national bank; in fact, they might well have been against it). But, contrary to Berger, this does not overcome the fact that what the framers of the Fourteenth Amendment *did* (knowing full well, no doubt, the already rich history of the other general phrases in the Constitution) was to lay out in the Constitution not specific rules or objectives but broad *principles:* "privileges" and "immunities," "due process of law" and the expansive right to "the equal protection of the laws." Thus, that the framers did not openly contemplate granting blacks the right to serve on juries equally with whites (a right which the Court recognized in 1879 in the *Strauder* case and which Berger, looking at the debates of 1866, deems "wrongly decided," p. 412) is both true and irrelevant. For what the writers of the Fourteenth did—one might even say intended—was to lay out bold principles of justice and constitutional right and embody them in a document which, to use Marshall's words, was meant to endure for ages to come. Even Justice Harlan, whom Berger rather consistently praises, could say in *Duncan* v. *Louisiana,* "The very breadth and generality of the Amendment's provisions suggest that its authors did not suppose that the Nation would always be limited to mid-19th century conceptions of 'liberty' and 'due process of law' but that the increasing experience and evolving conscience of the American people would add new [meanings]" (391 U.S. 145, 175; cf. Berger, p. 259). For each generation to apply the great principles embodied in the Constitution to new and varied objects and situations is not, as Berger believes, to make shambles of constitutional law. Rather, it is to act in the only way appropriate—to use the principles and goals of our fundamental law as directive and binding guides to our ever-changing condition as a nation.

Not only may constitutional formulations develop in response to *new* situations but also the internal logic of general principles of law and justice often lead us quite appropriately to reconsider and reevaluate situations prevalent in the past. That demanding "equal protection of the laws" for all citizens was never "thought" by the framers (and therefore could not have been "intended" by the framers) to apply to the status of women can be no final bar to its application in that area once the principle is finally and fully thought out. That the liberal egalitarian principle of the Declaration of Independence was not applied to slaves in 1776 can still mean, as Lincoln noted, that as the principle "broadens and deepens its influence" that it will prove to be incompatible with the very human servitude which gave Jefferson the time and opportunity to formulate the principle itself. That ideas do have consequences—and consequences which may go beyond or even against the immediate objects which gave birth to those ideas—is a fact having application well beyond the confines of constitutional adjudication. And while the Court can surely mistake the meaning of a broad constitutional principle (as in *Slaughterhouse*) or even subvert it (as in most of the racial cases of the 1880s), the Court's greatest function and perhaps its only real justification may be that it can help us, over time, elucidate our principles, apply them to new situations and give those fundamental principles the opportunity to unfold themselves in history. That the framers of the Fourteenth Amendment did not, in 1866, give to a branch of the central government the authority to overturn state malapportionment or to desegregate public facilities is obvious; that the principles written down in 1866 may never, therefore, demand those results is not so obvious.

Clearly, the line separating the proper application of constitutional principle and unwarranted constitutional revision is often narrow and the guideposts unclear. And, despite the basic thesis that the Court should be little more than a conclave of nine legal historians, Berger's book understands and depicts the real dangers involved in granting to the Court the unlimited right to fashion constitutional doctrine simply at will. The growing number of books and articles concerning the "imperial judiciary" attests to the fact that a reexamination of the proper role of the Court in a constitutional democracy is now both appropriate and necessary. The time surely has come for wide-ranging analyses of the Court in modern American life—investigations which will speak candidly about the ways in which the judiciary may have entered areas in which it has no experience, no ability or no legitimate business. The time has also come—and here Berger's instincts are fully correct—for a body of scholarship which goes deeper than the simple portrayal of the Constitution as a "living document" as if it were merely flexibility itself, without principle, meaning or solid purpose. But this analysis, which attempts to criticize the Court through an understanding which freezes the Constitution to certain historical particulars and no more, simply will not do, for it does not speak candidly about the Court as much as badly about the Constitution.

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Big Story: How the American Press and Television Reported and Interpreted the Crisis of Tet, 1968 in Vietnam and Washington. By Peter Braestrup. (2 vols. Boulder, Colo.: Westview Press, 1977. Pp. 740 + 706. $45.00; Garden City, N.Y.: Anchor, Doubleday, 1978. Pp. xviii + 606. $8.95, paper.)

Big Story argues that the American news media gave the public a basically false picture of the Tet offensive. Peter Braestrup attempts to show that the news media overreacted to the Tet crisis and presented it as a disaster for the U.S., when in fact it was a defeat for North Vietnam and the NLF.

Braestrup was himself a Vietnam correspondent for the Washington *Post* during Tet, and the strengths and weaknesses of *Big Story* are in many ways parallel to those of the news media he is writing about. As a gatherer of facts, Braestrup is thorough indeed. *Big Story* is based on extensive interviewing and a reading of almost the entire Tet coverage of the AP and UPI, the television networks, the New York *Times*, the Washington *Post*, *Time* and *Newsweek*. However, Braestrup's *analysis* has serious flaws.

Braestrup's basic observation about media content is correct: the news media did indeed present the Tet offensive as a failure of U.S. policy in Vietnam. But his style is so unremittingly contentious that Braestrup seriously overstates his points. With each example he lets his readers know he is scoring a point, and he often presents the "exceptions" with the same triumphant tone, as though they somehow proved his argument as well ("CBS managed . . . without. . ."). If one reads Braestrup's material carefully, one sees that it is often so mixed that his simple characterizations are clearly inadequate.

Braestrup's major criticism of the news media also contains a kernel of truth. The thesis that Tet was, at least militarily, a defeat for North Vietnam and the NLF is more plausible than most people realize, and the media did often make sweeping statements about the significance of Tet and presented dramatic incidents as though they somehow contained the "meaning" of Tet in microcosm, all with little solid analysis to back them up. But Braestrup himself treats complex questions of historical interpretation as though they were as simple as getting it straight whether the NLF did or did not actually enter the U.S. Embassy building on the first day of the offensive. "There is fairly broad agreement among historians today," Braestrup maintains, "that Hanoi suffered a military setback in the 1968 Tet offensive" (p. 119). But historians have barely begun to scratch the surface of the Vietnam war, and Braestrup does not cite a single historian to this effect. When historians do begin to analyze the Vietnam war, it seems likely that there will be disagreement about whether the question of "who won" the Tet offensive is even an important one to ask. After all, the U.S. was always able to kill more of the "enemy," hold any territory it chose, and keep the initiative militarily, yet North Vietnam and the NLF always seemed capable, politically and militarily, of continuing the struggle. Braestrup criticizes the media for getting the "score" wrong; it never occurs to him to question whether "scorekeeping" (which was always a top priority for the media) could really give people an adequate understanding of the Vietnam war.

Braestrup is always quick to tell his readers what he does and does not consider good journalism, but he is not always willing to spell out his reasons. He disapproves, for instance, of the attention the media gave to civilian casualties, calling it "old-fashioned appeals to the emotions" (p. 209). and "compassionate sensationalism" (p. 217). But he offers no justification for this view, except in purely personal terms: "As a former Marine in Korea, I was perhaps less shocked by war's waste and destruction than were my colleagues who were experiencing these for the first time" (p. xvi).

Braestrup's work suffers from his lack of knowledge about other research on the media. He presents Tet coverage as an "extreme case," and offers a jumble of miscellaneous explanations for it. In fact, it is easy enough to see the routines of news coverage at work during Tet. To take one brief example, the news media normally focus on day-to-day events rather than underlying processes. This means that a crisis like Tet almost always comes across as a sudden reversal in the course of events.

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The Presidential Experience: What the Office Does to the Man. By Bruce Buchanan. (Englewood Cliffs, N.J.: Prentice-Hall, 1978. Pp. ix + 198. $8.95, cloth; $3.95, paper.)

Taking his title seriously, Bruce Buchanan sets about to fill gaps in our understanding of the presidency by providing a comprehensive and systematic description and assessment of the psychological environment in which the occupants of the presidential office live and work. That milieu is pictured essentially as a constant one characterized by stress, deference, dissonance, and frustration. According to the author, these four acids of the presidential experience continually erode the moral habits and methods of truth-testing which incumbents bring to the office.

Drawing on the results of research in social psychology, Buchanan explores the behavioral consequences which may be expected in such an environment. Among those identified are tendencies to aggression, artificial inflation of one's own importance, weakening of truth norms, preoccupation with winning despite the cost, misinterpretation and misjudgment of people and events, reduction in physical and mental vigor, and delusion. Examples of each are found in the actions of twentieth-century presidents, leading Buchanan to conclude that the presidency as currently structured is a threat to political stability, democratic integrity, and effective administration.

The work concludes with two chapters which examine potential remedies. One investigates character-assessment as a device we might use to select presidents most likely to withstand the pressures of the environment. Drawing on self-concept theory and research, Buchanan provides valuable support for the notions developed by James David Barber in his paradigmatic book, *The Presidential Character*. Still, Buchanan will not confidently rely on character as a protector of his central values, for he is convinced that "on matters of profound personal importance to them . . . active-positives tend to behave like active-negatives" (p. 146). Thus he turns in the final chapter to institutional reform, but his conclusions are less than promising. The adoption of fundamental changes which might radically improve the environment, such as a plural presidency, is improbable. Those which might have some real political support, such as legislative question periods or censure motions, might reduce the effects of deference on the occupant of the Oval Office, but they would likely increase both stress and frustration.

The Presidential Experience is not without error. Using the models of staff organization identified by Richard Tanner Johnson in *Managing the White House* as his "Rorschach test" of a president's response to stress, Buchanan concludes that the formalistic model is a dangerous method of coping with stress and one which reveals a chief executive's inability to tolerate interpersonal conflict. While noting that an active-positive president might sometime "fall back on such an arrangement out of self-defense," the author contends that only Nixon and Eisenhower among modern presidents have implemented such a model (p. 45). In fact, however, Johnson described the Truman administration as a formalistic one (p. 51 ff.).

The more serious problem with this work is one which seems characteristic of such studies. Buchanan cites behavior as evidence illustrating his notions, but this evidence is not the result of systematic examination of presidential actions. This is particularly disappointing because Buchanan's notions are richer in their implications than the use of the data suggests. For example, he repeatedly emphasizes that the continual press of the four elements in the psychological environment makes them likely to produce aberrant behavior (pp. 25, 46, 58, and passim). That would indicate that instances of such behavior should occur, or should occur with more frequency, later in a president's tenure. Buchanan cites Roosevelt's court-packing plan as a major instance of preoccupation with winning despite the cost (pp. 111–16). Such action at the outset of the second term invites attention to subsequent behavior. The invitation is not accepted in this instance or elsewhere.

Despite these criticisms, *The Presidential Experience* is an important book which will provoke controversy among scholars and students. Written with style and energy, it is stimulating in its contemporary relevance, in its use of research findings from another discipline, and in its relationship to ideas developed by classic works on the presidency. Most important, it continues a line of research which, despite what may well be inherent limitations, deserves further pursuit. Buchanan has done us a service.

CHARLES H. MCCALL

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Decision for the Democrats: Reforming the Party Structure. By William J. Crotty. (Baltimore and London: Johns Hopkins University Press, 1978. Pp. viii + 318. $16.50.)

This book has two main ingredients: descriptions of the origins, activities, and products of the five "reform commissions" established by the national Democratic party between 1968 and 1975; and evaluations of those products.

The descriptions focus mainly on the McGovern-Fraser Commission (147 pages) and the O'Hara Commission (74 pages), with the other commissions—Mikulski, Sanford, and Winograd—mentioned only briefly (15 pages for the lot). But the descriptive *methods* are the same for all five. Crotty's accounts and explanations of events are based almost entirely on the public and semi-public record—commission hearings, memoranda, and reports; convention proceedings; newspaper accounts and commentaries. He mentions a few interviews with commission members and staff, but makes almost no use of them. This is particularly surprising in view of the fact that Crotty, as a consultant to three of the commissions, had unusual opportunities for gathering less accessible, and usually more illuminating, information. Even more surprising is the fact that he makes no use whatever of the massive collection of data from interviews with the delegates to the 1972 Democratic convention conducted by the Russell Sage/Twentieth Century Fund project of which he was one of the five principal investigators. He also does not mention the major book by Jeane Kirkpatrick based on those data.

Crotty's approach makes his book useful mainly as a convenient chronology of events and a compendium of names, dates, and occasional tabular material. Because he relies so heavily on what he has read, his analysis of why the commissions did what they did floats on the surface of events and his explanations are generally unconvincing.

This failure is most apparent in the frequent evidentiary and analytical gaps in his account of the McGovern-Fraser Commission, which occupies nearly half of the book. This judgment is not based on any inside knowledge I acquired as a member of that formidable commission—indeed, with even greater access than Crotty's at the time, I saw only the same visible minor fraction of the iceberg. But I have recently been reading Byron Shafer's Berkeley doctoral dissertation on the formation and implementation of the commission's rules. Shafer's analysis rests mainly on in-depth interviews with over 150 of the principal actors involved, and, while it would be unfair to fault Crotty for not having read Shafer, I must note that their analyses diverge at many important points and that in each case Shafer's is more convincing.

For one example, Crotty describes the commission's activities almost entirely in terms of what various commissioners said and did; Shafer shows that the staff played a highly active and partisan role and powerfully influenced the outcomes. For another, Crotty cannot bring himself to believe that an old pol like Lawrence O'Brien really favored the reforms or helped them very much; Shafer demonstrates that O'Brien not only favored them but played a critical role in getting the state parties to comply with them.

I have no comparable way of checking Crotty's accounts of the other four commissions, so I will note only that they all rest on similar kinds of evidence and analytical methods.

Crotty's evaluations of the commissions' products is highly favorable: they "managed to breathe new life into moribund party structures" (p. 272). This, of course, runs sharply counter to the view of some other political scientists, including me, that the new rules have contributed greatly to what Jeane Kirkpatrick aptly calls "the dismantling of the parties." Our differences with Crotty in this regard are probably of no great scholarly interest but the *grounds* for them may be instructive.

The critics of the new rules condemn them mainly because of their *perceived consequences*—for example, the proliferation of presidential primaries, the elimination of candidate-vetting by leaders and activists with a prime and continuing commitment to parties rather than to issues or candidates, and the consequent reduction of the parties to little more than passive arenas for conflict among entrepreneurial candidate organizations and special interest "caucuses."

Crotty does not defend the commission's "reforms" by attacking the veracity of such statements about their consequences—indeed, he barely mentions them. Instead, he praises the rules' *intrinsic fairness*, their increased commitment to what he tells us are his own prime values of openness, participation, and procedural fair play.

The differences come into sharpest focus in the two sides' very different evaluations of the 1972 national convention, the first major product of the McGovern-Fraser rules. The critics say it was extremely unrepresentative of ordinary Democrats; indeed, Kirkpatrick concludes from her interview data that the policy views of Democratic identifiers were much better represented by Republican delegates than by their Democratic counterparts. Crotty, as noted above, makes no mention of either the data or Kirkpatrick's findings. Instead, he presents a table (4.4) showing that the 1972 delegates included many more blacks, women, and youths than did the 1968 delegates.

The critics say that the convention's unrepresentative character led directly to its nomination of McGovern, whom they regard as the worst candidate and biggest loser in the party's history. Crotty agrees that McGovern ran a "hopelessly inadequate campaign," and that "many party regulars and state organizations never really took the national ticket seriously"

). 222); but in his view that was significant nly because it gave the anti-reformers an xcuse to launch a post-election campaign gainst the excellent new rules.

In sum, the rules' critics think that the 1972 onvention was a disaster. Crotty concludes (p. 49) that "the delegates . . . were orderly, elf-disciplined, and intensely concerned" and hat "the mood, the sense of order, the edication to work, and the outcomes represented a polar extreme from the unhappiness hat marked the [1968 convention]."

Who, then, is right about whether the ost-1968 "reforms" are good or bad? The nswer, it seems, depends upon which of two enerable maxims one chooses as a guide: "By heir fruits ye shall know them," or "Virtue is s own reward."

AUSTIN RANNEY

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he Powers That Be: Processes of Ruling-Class Domination in America. By G. William Domhoff. (New York: Random House, 1978. Pp. xv + 206. $10.00.)

For many years, G. William Domhoff's wit, nsight, and engaging prose style have given adical scholarship in America what little attraction it possesses. In *The Powers That Be* Domhoff continues to develop his thesis of ruling class hedgemony"—a thesis which he rst set forth in *Who Rules America* (1967) and *he Higher Circles* (1970) and elaborated on in *ats Cats and Democrats* (1972), *The Bohemin Grove and Other Retreats* (1974) and *Who eally Rules* (1978). Of course, all of these works directly challenge the prevailing pluralist deology of American political science. But Domhoff remains unrepentant: he continues to iew American politics as a struggle between wo social classes whose interests are rooted in he organization of production—a ruling class which owns and manages capitalist enterprise nd a working class which does not.

Domhoff is unimpressed with the traditional luralist argument that power can only be nderstood as a "process." He would be satisfied to infer power from "outcomes"—disproortionate wealth and income in the hands of he upper social class; their higher standing on a ariety of measures of the quality of life; their ontrol over major social and economic instiutes of society; and their over-representation n government. But in *The Powers That Be* Domhoff explicitly accepts "the Challenge" resented by pluralists to explain the *processes* "by which the ruling class in the United States dominates government and subordinates other social classes" (p. 9).

The processes of ruling class domination are classified as:

1. The special interest process, by which narrow short-run interests are advanced;
2. The policy formation process, by which general policies of the ruling class as a whole are developed;
3. The candidate selection process, by which the ruling class gains access to government;
4. The ideology process, by which ruling class beliefs are disseminated and reinforced.

Domhoff's description of the special interest process fails to rise above the standard anecdotal material in trickery, bribery, conflict-of-interest, and high-pressure lobbying that one finds in many muckraking accounts. Regrettably, his description of the candidate selection process is equally commonplace: electoral rules perpetuate a banal two-party system which avoids issues, discourages participation, and allows wealthy fatcats to manipulate candidates. It is not that Domhoff is wrong in his observations, but rather that others have described these processes more thoroughly and accurately.

Domhoff's best-developed model, and most intriguing supporting evidence, are found in his description of the elusive policy-formation process. The prestigious policy-planning groups are "at the center of things" (p. 64). Brief glimpses are provided of the composition and activities of the Council on Foreign Relations, the Committee on Economic Development, the Conference Board, the Business Council ("the Big Four of the policy network"), together with other "satellites and think tanks," including RAND, the Urban Institute, Resources for the Future, and the new Business Roundtable. Case studies of the activities of these groups provide both historical and contemporary evidence of their importance in overall policy development.

Domhoff's thinking about the ideological process is innovative, but his ideas on this topic are not as well developed as his ideas on policy formation. The ideological process is aimed at preventing the emergence of class consciousness among the masses and the development of serious alternative political programs. Individualism, free enterprise, competition, equality of opportunity, and related liberal notions are disseminated at all levels of corporate, governmental, foundation, and organizational life. Unfortunately, Domhoff decides that "the ideology network is too big to describe completely" (p. 173). We are left with only a few interesting vignettes about the shaping of public opinion (although a short essay on the Advertising Council is a gem).

Pluralists will be enraged by this book (and so will "the powers that be," if they ever read it). It is more provocative than it is conclusive; it is anecdotal rather than systematic. But Domhoff's many fans—both Marxist and non-Marxist—will welcome its appearance.

THOMAS R. DYE

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Scholars, Dollars and Bureaucrats. By Chester E. Finn, Jr. (Washington, D.C.: The Brookings Institution, 1978. Pp. xiii + 225. $11.95, cloth; $4.95, paper.)

The relationships between higher education and the national government which sank nearly $14 billion into it in fiscal 1977 are the concern of this very good book by a former research associate in the Brookings Governmental Research program who is now senior legislative assistant to Senator Patrick Moynihan. It is recommended highly to those willing to look up from the disciplinary workbench to consider the health of the academic enterprise upon which their specialty depends and to those political scientists now investing heavily in the new concentration on public policy.

Certainly the public policy field examined here is important and complex enough. The federal government has no higher education policy; it has only "haphazard bits of policies." Federal support of higher education is almost always a mean to an end, not an end itself—to procure research useful to other programs, say, or to advance some other social end such as expanding opportunity for the poor or fighting discrimination. Nowhere in the federal government is there a general responsibility for policy toward colleges and universities. The academy responds to scraps of policy designed for a wide variety of purposes to which it is incidental.

Is this good or bad? Finn makes a case for both. The lack of concentrated federal authority and rationalized goals has given the institutions a chance to foster variety and proceed with great autonomy and freedom. But the attendant problems are many, and Finn proposes some modest structural changes he believes would help.

The principal value of Finn's book, it seem to me, is his sophisticated and knowledgabl explication of practices and problems. He ha read widely, has understood what he read, an deploys his information judiciously. He is no dogmatic and he deals even-handedly with th possible consequences of various changes an the uncertainties which accompany any o them. He becomes controversial in that are where only angels perhaps could stand aloof the questions about how the federal government should assist higher education.

In the public-private controversy over thi policy, Finn's sympathies are clearly with th private interests. He views student assistanc correctly as having two aspects, the questio how best to expand access to higher educatio to categories of young people and the focus o a struggle among various kinds of institution over which ones shall receive the benefit flowing from enrolling federally subsidized students. He recognizes the dilemma involved in al welfare programs: shall the poor get steak o make do with macaroni? Where higher education is concerned, Finn is strictly a "let them eat steak" man. The poor must have a "choice"—i.e., to go to a high-priced institutio if that is what they desire. Finn advocates a two-tier schedule of assistance, the lower fo those who will settle for a home-state public school, the higher for those who insist on an out-of-state public or a private college. This would equalize choice and balance the public-private competition.

What if the public should raise tuition too, to take advantage of the federal largesse? Finn says that would be bad, but later he reports sympathetically on suggestions that tuitions in all kinds of institutions should be full-cost pricing, with the differences supplied by increased student aid. He believes that "the objection to higher public tuition was doctrinal and not practical" (p. 130). Perhaps that is right, but so too is the special value attached to private schools by those who love them. Proving that some policy positions are bottomed on values and emotions simplifies nothing. Let the author repeat to himself ten times: "The privates could all go public" and see where that hits him!

Finn scorns the idea of institutional aid to public and private schools alike, to use as they please, justified on the ground that higher education *per se* is in the public interest. This is the only solution ever to have the unanimous support of the higher education community and its failure in Congress was certainly not strictly on the merits. Neither is he consoled by private acceptance of half-measures, such as tuition offsets.

On the subject of the hated federal regulations, the book is judicious. Regulation of higher education as such has never even been considered. Laws aimed at other social goals affect higher education, and would without federal money. Institutions might try self-regulation or voluntary support of social reforms. Setting credibility for these efforts will be something else.

RALPH K. HUITT

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Approaches to Controlling Air Pollution. Edited by Ann F. Friedlaender. (Cambridge, Mass.: MIT Press, 1978. Pp. x + 465. $24.95.)

Lawyers generally favor standards and sanctions to control pollution. Economists generally favor some form of economic incentives. Lawyers call for the direct approach, while economists call for indirect, market-like mechanisms. This book, a collection of seven articles from a conference held at MIT in 1976, is a major contribution to the controversy about how best to reduce pollution—whether to use directive or incentive methods.

Helen Ingram, Richard B. Stewart, and Marc J. Roberts and Susan O. Farell assess aspects of the existing standards and sanctions approach; A. Michael Spence and Martin L. Weitzman compare the use of standards and sanctions with the use of incentives; Daniel L. Rubinfeld and Donald N. Dewees comment on the problem of assessing the benefits and costs of pollution control, a problem that confronts both pollution reduction methods; and Edwin S. Mills and Lawrence J. White argue that major benefits would accrue if effluent fees replaced direct standards for the control of auto emissions.

The editor, Ann F. Friedlaender, maintains that there is agreement that uniting the two approaches—combining standards with the use of economic incentives—is the best alternative. Disagreement exists, however, about how economic incentives should be determined and what purpose they should serve. Economists like Spence and Weitzman argue that incentives should be determined by estimates of how much damage pollution causes. According to economists, the purpose of incentives are to internalize externalities and to restore market-like efficiency. On the other hand, William Drayton, a lawyer and EPA administrator, argues that economic incentives should be determined by how much a firm saves from not complying. The purpose of the incentives would be to bring violators into compliance, not to maximize market-like efficiency.

According to Drayton, incentives are an enforcement method. According to Spence and Weitzman they are means to increase overall economic efficiency. Friedlaender comments:

> Unless the marginal damages are equal to the marginal costs of cleanup, the two systems will lead to different results. In particular, if marginal costs of cleanup rise rapidly as zero discharge is reached and if marginal damages rise slowly with movements away from zero discharge levels, it is likely that the Connecticut plan (advocated by Drayton) will lead to more cleanup than the standard-fee plan because the penalties for noncompliance will be greater under the former than the latter.

In 1977, the EPA, under new amendments to the Clean Air Act, was granted authority to introduce a version of the Connecticut Plan, which uses incentives as an enforcement tool, not as an efficiency-maximizing method. This book provides interesting insights into the reasons why the Connecticut Plan was adopted and what it is likely to accomplish.

This volume on alternative approaches to air pollution control is useful not only for the insights it provides about the use of incentives. Its conclusion—in the form of comments by J. Clarence Davies, James E. Krier, Allen V. Kneese, and Roger Strelow—provides an interesting catalogue of alternative reform proposals. Davies, for instance, argues that the scientific basis for all standards is inadequate and the EPA's Office of Research and Development should be reorganized so that it is more responsive to the needs of EPA's line programs. Krier maintains that because the 1970 Clean Air Act imposed vast economic and social costs for relatively small improvements, a management-based standards system that would permit regional variations is appropriate. For anyone interested in alternative means to achieve regulatory ends, this volume provides serious and sophisticated analysis.

ALFRED A. MARCUS

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Protest at Selma: Martin Luther King, Jr. and the Voting Rights Act of 1965. By David J. Garrow. (New Haven: Yale University Press, 1978. Pp. xiii + 346. $15.00.)

This book focuses on the 1965 Voting Rights Act, which David Garrow characterizes as "a legislative enactment that was to stimulate as great a change in American politics as any one law ever has" (p. 236). As background, Garrow details the increases in southern black voter registration from 1940 until the eve of enactment. Later chapters trace the impact of the legislation in terms of voters listed by federal referees appointed pursuant to the act, blacks who registered with local registrars, the election of black office holders and the quest for black votes by white candidates.

The bulk of the book describes the events which led up to passage of the 1965 Act and analyzes the importance of the Selma protests in mobilizing congressional support for President Johnson's emerging voting rights legislation. The pivotal event in the months of civil rights activity was, of course, the March 7 attack by law enforcement officers on peaceful marchers. The marchers were setting out from Selma to Montgomery to dramatize demands for federal legislation which would remove discriminatory obstacles to black voting.

If *Protest at Selma* were no more than a descriptive treatment, it would still be a valuable source book for anyone interested in the events of the period or in the growth of black voter registration. Garrow's research and documentation are meticulous. He has carefully culled the journalistic treatments of the events. With these he has integrated scholarly analyses, publications of activist groups, congressional documents, and papers found in the Lyndon Johnson archives. I suspect that virtually no source relevant to the topic has escaped Garrow's painstakingly thorough research. These sources are all included in the extensive, interpretative footnotes.

Much of chapters 2 and 3 will be useful to anyone interested in a chronology of events in 1965 which culminated in the passage of the Voting Rights Act. Garrow presents a daily diary which includes the activities of the demonstrators in Selma, Birmingham and other communities, the responses by the president and members of Congress to the protests as they went about shaping the legislation, and the court battles fought over issues such as the right to protest and to register to vote. Trying to report simultaneously on the events along several fronts might, if attempted by a less skilled author, result in disjointed flitting from one scene to another. However, Garrow is able to move his reader from Brown's Chapel in Selma, to a federal district courtroom, to the anterooms of Congress, to White House strategy sessions, and back again, without confusion. Indeed, the author uses the day-by-day style, as have several popular writers, to lend drama to a sequence of events which, at least in their broad outline, are already known to the reader.

While not discounting the utility of this book as a reference for the period, I suggest that it is even more valuable for its theoretical contributions. The 1965 Selma protests are compared with those in Birmingham two years earlier. Garrow offers reasons for the more direct response of federal policy makers to the Selma activities. Elsewhere the author demonstrates the learning process undergone by Martin Luther King, Jr., and the SCLC as they moved from earlier unsuccessful efforts in Albany, Georgia, to the Selma strategy of using peaceful protests to evoke a violent response which attracted media coverage and stimulated congressional support.

This exceptional book also explores the impact of media coverage on congressional attitudes and the link between constituents and legislator behavior. Legislators' comments published in the *Congressional Record* are reviewed to determine the source of their information and concern. Garrow controls for the proportion black in the legislator's constituency in an effort to determine whether legislative support of the protestors was triggered by conscience or simply constituted position taking. While I do not find the evidence compelling, the author concludes that proponents of relief for the protestors were not responding to actual or anticipated constituency demands.

Protest at Selma is a fine piece of scholarship. Its publication gives David Garrow much brighter career prospects than are enjoyed by other political science graduate students.

CHARLES S. BULLOCK, III

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Essays on the Constitution of the United States. Edited by M. Judd Harmon. (Port Washington, N.Y.: Kennikat Press, 1978. Pp. 202. $12.95.)

An unanticipated bonus of America's bicentennial celebration (commencing in 1976 and not likely to conclude before 1989) has been the serious reexamination of the Constitution's origins, development and significance. For at least a generation, that quest had seemed virtually abandoned by a political science that had confined the study of public law almost exclusively to judicial process or behavior.

This slim volume is a welcome indicator of a renewed professional interest in American constitutional interpretation. Originating as a bicentennial lecture series at Utah State University, *Essays on the Constitution of the United*

States reflects the matured judgments of "an all-star cast" of scholars who have kept the *vestal flame alive. An insightful introductory* essay by the editor, M. Judd Harmon, sets the stage nicely, calling attention to the remarkable common thread woven through these eight quite disparate contributions—namely, a seemingly "greater respect for the Constitution and confidence in its capacity to resolve problems than has existed for many years, perhaps greater than has ever existed" (p. 4).

The essays themselves begin with Alpheus T. Mason's impressively designed and written framework of conceptual underpinnings that comprise America's political heritage. Citing John Quincy Adams' observation that the United States had been founded not as a "democracy most simple" but rather "the most complicated government on the face of the earth," Mason prefers the term *free government.* "More easily described than defined," he notes, "it involves a complexus of controls designed to temper together into one consistent work the sometimes opposite, sometimes complementary, elements of liberty and restraint" (p. 19).

The Bill of Rights serves as the main focus of two essays—those by the late Herbert J. Storing and by David Fellman. Storing deals with the origins of the first ten amendments to the Constitution, adding a fresh dimension to the conventional wisdom. In Storing's view, James Madison's insistence on sponsoring a particular set of amendments was the final stage in the remarkably successful early federalist strategy to secure an effective national government. This stroke headed off more far-reaching anti-federalist restrictions, as well as—in the absence of congressional action—an ominous portent of a state-called general convention under Article V that would propose disabling amendments (striking a modern note here). Fellman's essay next briefly summarizes the historical development that nationalized basic civil liberties via the Fourteenth Amendment.

The Supreme Court and the political process receive particular attention in three separate essays. With his customary skill at synthesis, C. Herman Pritchett discusses judicial supremacy from Marshall to Burger, pronouncing the Supreme Court's record, on balance, a success. "The courts," he reminds us, "are accessible to any individual or group with a valid case or controversy. Minorities that are too small to be effective in the political arena may find the courts are the only public agencies willing to listen to them" (p. 110)—with racial desegregation and reapportionment as major examples. This same point—that judicial intervention is most justified when the path of political change is blocked—is reiterated by Henry J. Abraham. But his essay goes on to explore deftly the limits of judicial policy making, cautioning: "The Court is much better at saying what the government may not do than in prescribing what the government must do and how it must go about doing it" (p. 71). The Supreme Court's essential power still remains the power to persuade. The policy area traditionally most immune from judicial involvement—foreign affairs—receives thoughtful treatment in a fine essay by Louis Henkin. Noting with approval recent congressional assertion of authority, Henkin still concludes that neither Vietnam nor Watergate has significantly changed congressional handicaps in foreign affairs vis-à-vis presidential control.

By far the longest essays, aimed more at a professional than general audience, are authored by Martin Shapiro and Walter F. Murphy. Shapiro's piece on "The Constitution and Economic Rights" will raise eyebrows (and hackles) with such statements as this: "Those liberal commentators who applaud the activism of the Warren Court would do well to remember that the economic theories of the turn-of-the-century Court were as public interest-oriented, more clearly articulated, better scientifically grounded, and show greater survival value than the sociological, psychological and criminological theories that shimmered just below the surface of much of what the Warren Court did" (p. 80). But Shapiro's target is broader than the Warren Court. He takes on most of the scholarship in American constitutional interpretation since the 1930s, which he regards as a virtually monolithic approach, one he labels "the New Deal school" which taught judicial self-restraint in economic regulation but a special judicial responsibility to protect personal and civil rights. When Shapiro gets past this overdrawn historical backdrop, however, he develops a cogent and compelling analysis. There was a continuity in substantive approaches by the Supreme Court after 1937 that went beyond the oft-noted shift from economic concerns to personal rights. Shapiro argues convincingly that there was also a judicial continuity over a wide spectrum of *economic* concerns, as well as an increasingly recognized interrelationship between property and personal rights. "In any event," he concludes pointedly, "it is now fruitless to debate about whether the Court can or cannot involve, or has or has not involved, itself in constitutional economic policy making since the New Deal. It can. It has. It does. And there is no sign it is going to stop" (p. 98).

In the final essay, Walter Murphy probes the art of constitutional interpretation by concentrating on three questions in particular: (1) What are the scope and meaning of "the Constitution"? (2) What is the Constitution's relationship, if any, to some higher law? (3) What is the ranking of the various values that the Constitution seeks to promote? These far-reaching questions are probed with sophistication and scholarly care. Murphy concludes that human dignity is *the* fundamental value of the American constitutional system. Acknowledging the shortcomings of all such normative formulations, he sums up his thesis in these words: "In essence ... it means that the individual, as a person, is the basic unit of legal—and moral—accounting; that government must respect all persons, in Kant's terms, as ends rather than as means; and that each person has equal claim to that respect, not because government so deigns but because we share a common humanity" (p. 157).

This book reflects the diversity and potential of a subject that calls for further scholarly exploration as we enter the final decade of the American Constitution's first 200 years.

GORDON E. BAKER

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Congress and the Budget. By Joel Havemann. (Bloomington: Indiana University Press, 1978. Pp. viii + 247. $12.95.)

Joel Havemann has concluded that the new congressional budget process is alive and well despite the fact that neither spending totals nor priorities have changed greatly since its enactment. In arriving at his conclusion, Havemann takes us through the development of the 1974 Budget Act and its first two years of implementation. He relates the legislative battles over jurisdictional turf and spending priorities. Much of the fighting during the first two years was over the integrity of the process itself. If Congress had been unable even to pass the concurrent resolutions (setting overall targets for revenue and spending) that are the heart of the process, the whole project would have been a failure.

One of the areas of greatest interest to students of Congress is Havemann's analysis of the establishment of the two budget committees. How was it possible to establish new committees that would likely threaten other power centers in the two houses? Havemann finds quite different patterns of accommodation in the House and Senate. Chairman Muskie of the Senate Budget Committee was much more aggressive in defending his interpretation of the congressional budget on the floor than were the House chairpersons (Ullman, Adams, Giaimo) who tended to be more passive and accommodating to the wishes of other committees and the House leadership. This was due in part to the status of Muskie in the Senate and the focus of his committee's analysis. The Senate Budget Committee tried to concentrate on overall totals of revenue and spending, while the House committee scrutinized more closely individual spending items. Partisan battles continually plagued the new process in the House, but Senate Budget Committee proposals were easily successful on the floor.

Judging the impact of the new budget reform is filled with ambiguities. In one sense it is an impossible task, since no one can say what Congress would have done had the 1974 Budget Act not been enacted. One approach Havemann takes is to compare budget outcomes, in terms of fiscal policy and spending priorities, with what Congress did in the five years immediately preceding budget reform. His analysis shows that Congress did not alter the president's budget proposals significantly more with the new process than it had before the reform.

Certainly conservatives were disappointed that the new process did not lead to reduced spending, and liberals were disappointed that it did not eliminate many tax expenditures or increase support for social programs. About the only people who claimed a victory were those whom Havemann calls the "proceduralists" (p. 201), that is, those who were committed to the successful implementation of the new process and felt that it had to be ideologically neutral or it would be sabotaged by those who felt it was biased against them.

But what about those who argue that any reform is meaningless unless outcomes change? What difference does it make if we have two new committees, a tighter timetable, and a Congressional Budget Office that provides more information and better analysis if, in the end, we have the same winners and losers in the budgetary arena? Havemann argues that what the new process does is to force Congress to confront openly the consequences of its decisions when it votes on total revenue, spending, and deficits. Before 1974 the national budget turned out to be the sum of the separate appropriations bills which were passed. Fiscal policy has now become an explicit element of the congressional debate over the budget. But it would be naive to think that a new set of procedures could force Congress to do what it does not want to do. The same fragmenting forces for spending are still operating as they always were.

One reason why the major outcomes of the process were not significantly different was that the budget committees (with a few exceptions in the Senate) were unwilling to assert themselves by challenging other committees on the floor. According to Havemann, this passive strategy was calculated to ensure the survival of the process—and the budget committees. It was accommodation in the short run aimed at consolidation of a stronger base in the long run.

One possible direction of budget committee power is the potential for centralizing budget control in the two houses. Budgeting can be seen as a constant struggle between the fragmenting thrust of many claims and the central controllers who have to say "no." If the process survives and the budget committees are becoming stronger (and evidence indicates that they are), they may well play a role similar to OMB in enforcing central control and saying "no" to the always unlimited claims on the fisc. This controlling role will become increasingly important as political pressure is applied to decrease spending, and particularly if Congress is forced to adopt some budget-balancing device.

Joel Havemann is an editor of the *National Journal* who has covered the new budget process from its inception. His book is timely and extremely well written. Political scientists will not find analytic models or the systematic integration of his analysis with previous research on the behavior of committees in the congressional system. What they will find is a sophisticated analysis of the behavior of the individuals and institutions involved in the new congressional budgetary process. Havemann's access to the major participants infuses the book with many insights unavailable from the public record, and it tells us much about congressional behavior. If you want to know what happened in the first two years of budget reform, this is where to find out.

JAMES P. PFIFFNER

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National Party Platforms. Vol. 1, **1840–1956;** Vol. 2, **1960–1976.** Edited by Donald Bruce Johnson. (Urbana: University of Illinois Press, 1978. Pp. xiii + 1035. $35.00.)

This set is the latest edition of a basic reference work which was first compiled by the late Kirl H. Porter in 1924 and has been repeatedly updated by Donald Johnson in recent years. These volumes continue their past contribution of making available in one place the policy documents of all major and minor American parties since 1840. The latest version goes beyond this bibliographic service, providing a number of new and useful features.

For libraries, an important new aspect is the separation of the series into two volumes, so that only the second will need to be replaced with each quadrennial outpouring of party rhetoric. For scholars, the addition of subject and name indexes are important new features. One gains a comforting sense of historical continuity from the fact that taxation has been the subject of electoral dispute from the first to the most recent Democratic platform. At the same time, we gain some sense of the passing of time by contrasting such later entries as "sociology of science" and "abortion" in recent platforms with earlier entries such as "saloons" and "Christian Sabbath." However, the index would be more valuable, I believe, if it were combined in one place, rather than divided between the two volumes. Otherwise, this work is easy to use, well designed typographically and includes a convenient summary of candidates and election results heading each four-year section.

The principal use of these volumes, obviously, is for reference purposes, and no new research findings are claimed. Still, the very appearance of these documents implies their continuing significance in American politics. Platforms have grown greatly in length over the years. Almost half of the total pages are devoted to party manifestos dating from 1960. A specific comparison is provided by the Democrats, who required only nine short resolutions to summarize their principles in 1840, but took 30 times more space to present their policies in 1976. Thes echanges reflect the increased complexity of American society, the increased reliance on the federal government and the presidency to cope with this complexity, and possibly the greater import given to the platforms themselves.

In a brief preface, Johnson discusses some of the significance of platforms: as statements of party principles, as criteria for judging an incumbent administration, as catalysts for party factions and interest groups, as aids to the voters and as guides to government action. That platforms are taken seriously is indicated by the continuing attention given to them by politicians (even those, like George Wallace, who deprecate them). This authoritative collection will enable researchers to give them the further scholarly attention they deserve.

GERALD M. POMPER

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The New American Political System. Edited by Anthony King. (Washington, D.C.: American Enterprise Institute, 1978. Pp. 407. $6.75, paper.)

Given a charter by the American Enterprise Institute, Anthony King recruited nine colleagues to join him in a consideration of changes in American politics since 1960. The result is a marvelous book, not least because of the distinguished company assembled for the task. Samuel Beer begins with an argument that neither the technocratic nor the romantic revolts of the sixties provided a coherent body of ideas to replace the New Deal philosophy of earlier decades. Fred Greenstein compares the Ford-Carter presidency to that of the Roosevelt-Truman-Eisenhower period. Hugh Heclo writes of the specialized issue networks that have arisen, and of the problems these pose for political administration. Samuel Patterson considers changes in the constituencies represented in Congress, in congressional organization, and in decision making. Martin Shapiro compares the Warren and Burger courts as policy makers. Austin Ranney reviews variations in the electorate, in presidential party organization, in party rules, and in the consequent ability of the parties to respond effectively to the initiatives of other actors. Jeane Kirkpatrick arrays data on electoral activity against frameworks provided by three leading voting models. Richard Brody suggests that we are better able to account for the more demanding forms of electoral participation than for variations in election-day turnout. Leon Epstein deals with the increasing number of policies being administered by the states, and the consequences of categorical and general purpose grants to subnational units of government. Anthony King sums up with an argument that one cannot build effective coalitions out of grains of sand. These are lucid essays by seasoned scholars.

I think I learned most from the chapters written by Samuel Beer, Hugh Heclo, and Martin Shapiro—in part because of the skill with which they were written, and in part because my own work has not dealt with the role of ideas and the executive and constitutional arrangements through which they are given expression. I suspect many readers will profit from the able summaries of subjects that have not been in the forefront of their attention. The other half-dozen essays were all written with real authority, and bear the stamp of their authors' personal views as well.

The New American Political System should find considerable classroom use. It ought not to be used alone except with knowledgeable students. It presumes the reader knows a fair amount already. But at $6.75 for 400 fact-filled pages, it is a bargain, and used along with a good text, it will give able students a chance to focus on the question of political change.

Whether American politics is "new" is a question that has been asked for a long time. (See, for example, Arthur Holcombe's 1933 *The New Party Politics* or Paul Douglas' 1932 *The Coming of a New Party*.) Anthony King's answer to this time-honored query is that "a strong case can be made out that the changes that have taken place in the American political system are both profound and widespread in their implications—that it is no mere rhetorical exaggeration to describe the system as it now functions as new, certainly in a number of crucial respects" (p. 381). If this answer sounds hedged, it should; the question is exceedingly complex.

In fact, many of the authors stress continuity. Fred Greenstein writes: "In any institution it is difficult in the short run to distinguish between permanent changes and changes that will turn out to have been only ephemeral. Many changes in the post-Eisenhower presidency were thought to have been permanent at the time but from the perspective of the late 1970's appear to have been rather drastic zigs and zags in patterns that had been established during the first three modern presidencies" (pp. 61–62). Samuel Patterson, who has the most detailed statement of what is meant by change, says: "Congress is not easy to analyze holistically. It is a subtle and complex collective decision-making body with intricate linkages to its constituencies and to other governmental agencies. . . . Changes tend to be both small in scope and large in number. Probably it is not too much of an exaggeration to say that Congress changes persistently. The constancy of change and the incremental character of change make it difficult to acquire a firm grasp of the meaning of change" (p. 130). Martin Shapiro compares the transition from the Warren to the Burger Court with that between the Marshall and Taney Courts: "In both instances the partisan political setting of the transition leads to overstatements of the differences. In both instances the new justices have somewhat different policy preferences from the old and thus often reach somewhat different resolutions of particular cases. In both, however, the new Court receives and accepts the basic increments to judicial power and the basic theory of judicial review established by the old" (p. 203). Samuel Beer traces the technocratic confidence of the 1960s back to Francis Bacon, and points to the affinity between the Port Huron Statement and the romantic views of Wordsworth.

Questions can also be raised about the novelty of some of the analyses stressing change. Hugh Heclo writes: "In the old days—when the primary problem of government was assumed to be doing what was right, rather than knowing what was right—policy knowledge could be contained in the slim adages of public administration" (p. 103). Heclo's thesis that increasing complexity and specialization has given more influence to subject matter specialists is far more fully developed, but one may ask whether his fundamental argument is any different from Robert C. Wood's 1960 insight: "Complete as the reversal in the status of the professor among the politicians may be, there is nothing especially mysterious about it. . . . Up until the New Deal most domestic policy issues could be presented as simple moral choices. . . . Today the awesome complexity of national policy is immediately apparent. . ." (*New York Times Magazine*, 15 May 1960, p. 15). Anthony King holds that American politics has become atomized. "The politics of the 1930s and the 1940s resembled a nineteenth century battlefield, with opposing armies arrayed against each other in more or less close formation; politics today is an altogether messier affair, with large numbers of small detachments engaged over a vast territory, and with individuals and groups frequently changing sides" (p. 372). Opposing armies in close formation? In 1940, Pendleton Herring was writing that political parties had become less significant as rival organizations had grown. "This development has reached vast proportions today. It presents a vascular politics with an infinite number of arteries penetrating down into the community. Self-government demands free circulation of individuals' impulses. This calls for a varied network throughout the social structure. Disciplined parties, clearly differentiated, result in a hardening of these arteries" (*Politics of Democracy*, p. 110).

The difficulty of asking whether politics is "new" is that it forces a simple dichotomous answer to a very complex question. What is needed instead is a thorough understanding of the phenomenon in question, a sufficient knowledge of the antecedent state, an ability to sort out the threads of continuity, and careful specification of the conditions leading to the changes that have taken place. Happily, the pages of *The New American Political System* are filled with materials fashioned to meet these needs.

JOHN H. KESSEL

Ohio State University

Federal District Judges: An Analysis of Judicial Perceptions. By William I. Kitchin. (Baltimore: Collage Press, 1978. Pp. 225. $8.70, paper.)

Using a limited form of role analysis, William Kitchin conducted 21 lengthy interviews with U.S. trial judges throughout four circuits. He employed a "purposive, range sample" rather than a random sample of judges and said that his goal was "not to test hypotheses but to generate them, not to provide a comprehensive picture of the perceptions of district judges but to gain insights as to what those perceptions are." The first substantive chapter discusses the significance of whether judges possess "process orientations or result orientations," and this is followed by a chapter on how the district judges view their colleagues on the appellate court bench. A third substantive chapter outlines the judges' views on the degree to which they are bound by past precedents, and the next chapter follows up on this theme by addressing innovation on the district court bench. The following chapter focuses on the administrative aspects of the judicial role, and a final chapter presents the author's conclusions.

There are several strong and very positive elements in Kitchin's study. First, it deals with quite an important topic about which we have very little systematic empirical data. There are very few books indeed devoted exclusively to the lower federal judiciary and no more than a score of truly good articles dealing with "behavioral" phenonema. Thus, Kitchin's book comes as a welcome addition to the literature on this significant but little-studied subject. Two chapters in particular stand out for their interest and utility. The first is Kitchin's chapter outlining the areas in which the judges believe they have the greatest capacity for innovation, that is, those areas in which their personal values and ideals are more important in the decision-making process than *stare decisis*. A second chapter which stands out is the one entitled "The Administrative Role of U.S. District Judges." This chapter fits well into the developing body of literature on judicial administration.

Kitchin is also to be complimented on his clear, terse style and on the honest manner in which he presents his empirical data and conclusions. He is quick to acknowledge the limitations of his research and to admit errors in the way he developed some of his interview questions. Such information is useful for students planning similar inquiries.

There are also several other shortcomings that strike the critical reader. One is that while Kitchin duly acknowledges (in Chapter 2) that his is a non-random sample not necessarily typical of district judges in general, he often seems to forget this limitation throughout the rest of the book. Despite occasional caveats such as "assuming the reliability of the data," Kitchin tends to generalize as if he were speaking about federal district judges as a whole rather than about an admittedly non-random sample comprising only about 5 percent of the universe.

Second, Kitchin sometimes appears to fall into the trap of believing the world is as his interviewees describe it, or at least of not subjecting the judges' comments to insights that social scientists possess. For instance, at one point Kitchin warns against using Shapiro's model which describes justices "as political actors playing a policy-making role." Kitchin says: "District judges simply do not see themselves in political or policy-related jobs." Perhaps the judges do think of themselves as nonpolitical, but political scientists have long recognized that judicial decisions are very much a part of public policy and the political process, and we should not accept mutely the judges' protestations to the contrary.

Finally, Kitchin offers a set of supposedly testable hypotheses in his last substantive chapter. To me they sound like no more than ordinary conclusions, and one wonders why Kitchin elevates them to the level of rigorous hypotheses which lend themselves to operational definitions. For example, H_{11} posits that "judges of all levels of the federal judiciary consider protecting liberties and maintaining the separation of powers system to be important parts of their roles." Such is probably a valid observation, but I would hardly call it a rigorous, testable hypothesis.

The above criticisms are quite minor, however, in contrast with the total scope and quality of Kitchin's book. It is indeed important reading for any student of the federal judiciary in the United States.

ROBERT A. CARP

University of Houston

Where Have All the Voters Gone? The Fracturing of America's Political Parties. By Everett Carll Ladd, Jr. (New York: Norton, 1978. Pp. xxiv + 86. $7.95, cloth; $3.95, paper.)

In *Where Have All the Voters Gone?* Everett Carll Ladd, Jr. argues that the political parties must be revitalized and that a necessary element of that revitalization is the scrapping of recent party reforms. The essay was not written for an academic audience (the bulk of the work initially appeared in *Fortune*). Nevertheless, the logic of his thesis is compelling.

Today, unlike years past, most Americans believe that their government is out of control and non-responsive. A partial explanation for this belief is that the political parties are no longer fulfilling their obligation to aggregate the interests of the people. A party system that effectively aggregates interests must be competitive (the in-party must either offer policies that the public prefers or be ousted by the out-party), representative (the parties must offer policies that appeal to their coalition of constituents) and organized (the parties must take positions on issues so that the people can vote for or against those positions and the party can then, once elected, translate the positions into government policy). At present, the parties in the United States fail to compete, represent and organize.

Instead, the United States has a one-and-a-half party system. The half party, the Republicans, directs its appeals toward a distinct minority of the middle class. Hence, the Republicans are not strong enough to control Congress or for that matter more than a handful of states. Yet the Democrats (the party of everyone since even most conservatives in America accept the programs of the New Deal) are unable to bridge the schism between the Old Class Democrats (the New Dealers) and the New Class Democrats (the present-day liberals). Hence, their party membership is deeply divided on new issues such as aid to minorities and protecting the environment. During well-publicized elections, particularly the presidential contest, the Democratic coalition splits and the Republican candidate becomes competitive provided he supports the New Deal.

Recent party reforms exacerbate the difficulties by opening up the nominations to the control of the rank-and-file members who tend to be more interested in choosing candidates who reflect their own issue positions than in winning the presidential election. Since the rank-and-file Republicans are more conservative and the rank-and-file Democrats are more liberal than is the population, the one and a half parties are becoming less and less representative.

In order to preserve the endangered parties, control of the parties must be returned to the professional politicians who in order to win the election usually pick candidates who appeal to a majority of the voters. If the selection of the president were placed once again in the hands of the party leaders, the parties could once again compete, represent and organize.

Ladd's argument is obviously not new. Indeed, it is one of the more often-used rationales for preventing the inclusion of new groups in a decision-making arena: in order to protect the people from their folly, decisions must be kept in the hands of an elite. Although the book contains new elite interview materials, there are no new interpretations and no new theories. It would be unsuitable for upper-division classes.

The book does have its strong points. It is well written, the data presentation is excellent, and the summaries of the problems in the Republican and Democratic parties are devastating. Finally, the essay is provocative. To accept Ladd's conclusions one must believe that the political parties can serve a useful function, that elected officials can control governmental policy, that political party machines served the citizens better than other alternatives, and that the content of governmental policy is more important than providing citizens with the opportunity to participate in determining that content. Thus, controversial assumptions underpin a tight logical argument for making the party nomination procedures less democratic. Further, the argument is articulated by a respected member of the academic elite for a business executive elite. For this reason, I highly recommend the book as either a supplement to an introductory American government course or as a provocative introductory essay for a political parties course.

JOHN R. VAN WINGEN

University of Southern Mississippi

Law and Identity: Lawyers, Native Americans and Legal Practice. By Linda Medcalf. (Beverly Hills, Calif.: Sage Library of Social Research, Vol. 62, 1978. Pp. 147. $14.00, cloth; $6.95, paper.)

It is much easier to denounce liberalism than it is to rectify its errors. It is much easier to identify the destructive aspects of a life permeated by liberal values than to describe constructive alternatives. It is much easier to decry the reification of law than to write a book that avoids reification. What is it about liberalism or legalism that draws denunciation yet eludes either convincing prescription or effective solution? I suspect that in many cases neither liberalism or legalism is the real target. In many cases an attack on either is really an attack on capitalism. Politics and law are on the surface, economics is underneath. We say liberalism or legalism when we really mean capitalism because we really don't have a political economy which is worked out in sufficient detail to explicate their linkages. Yes, there is Marx, but somehow Marx has rarely been either well understood by American social scientists or well used.

What does all of this have to do with the new short book by Linda Medcalf? I suspect that a close reader would answer "everything and nothing." Everything, because for Medcalf liberalism is the enemy and law its malevolent instrumentality. Liberalism destroys human possibility and legalism legitimizes that destruction. The dirty work of liberalism and legalism is done by lawyers. As Medcalf sees it, even the "good guys" are "bad buys." Radical lawyers are, according to Medcalf, more lawyers than radicals. Their language is the language of legal rights. Although they seek liberation, they are still slaves to a pattern of thought that defines human problems in terms of rights and duties. Medcalf argues that there is no real freedom in liberalism or legalism.

Is Medcalf a Marxist? It's hard to tell from the body of the book, and that is part of the problem. I think, however, that her ideology is more complex and interesting than perhaps even a well worked-out Marxism would be. Perhaps it would be more accurate to say that the book is humanistic and therefore impatient with capitalism without being clear about either its perspective or its impatience. That lack of clarity is, at base, what is so frustrating and, at the same time, intriguing about the book.

Medcalf's book is based upon a study of the small population of "radical" lawyers dealing with the problems of Native Americans. It is a study that is self-consciously but not conscientiously theoretical. Her book is intended to be a case study in the adequacy of liberal legalism. What she wants to show is that even the best intentions cannot overcome the curse of liberalism and the routines of legalism. Her question is how far and where does legal liberalism lead as a developmental strategy for Native Americans. Her answer is that it leads in the wrong direction, although she never identifies what the right direction would be.

Medcalf describes the strategies of the lawyers whom she studied as being threefold. They attempt to cope with the problem of poverty and powerlessness among the Native Americans by first securing their claims to their land; second, by establishing an effective structure of public authority to protect those claims and promote economic development; and, third, by identifying a set of rights which must be accorded to protect Native Americans from their indigenous, tribal governments. Liberalism recreated property, government, rights for Native Americans. Her view, however, is neither an accurate reconstruction of the political theory of Lockean liberalism nor the history of American liberalism. What really troubles her is that liberal legalism as an ideology of development inevitably leads Native Americans away from values that are both more authentic and more humane.

What values? This is the unanswered question. It is easier to tell what Medcalf dislikes than what she thinks is lost from the experience of Native Americans by pursuing the strategy being pursued by lawyers on their behalf. She deplores the depersonalization of politics, the ethic of efficiency, individuation, contest, adversariness, the idea of life as struggle, as power or as control. She seems to admire community, cooperation and unity. Yet we never hear the voice of Native Americans. We never find out what they want or what their experience would hold out as an alternative to the things Medcalf deplores. Medcalf's book is not well served by the absence of that voice.

Finishing Medcalf's book, I confronted a question I face in my own work and never answer fully. We seem so much better at criticism than construction, at stating what fails and why than figuring out what works and how to attain it. In moments I wonder and worry as much about myself and my work as about the books I read. Yet it isn't often that I'm driven to think such thoughts, and even a flawed book is valuable if it can call forth the right questions. How much better it would be to suggest plausible answers.

AUSTIN SARAT

Amherst College

Labor at the Polls: Union Voting in Presidential Elections, 1952–1976. By Jong Oh Ra. (Amherst: University of Massachusetts Press, 1978. Pp. x + 182. $15.00.)

Jong Oh Ra has written a very interesting book of genuine value to scholars seeking a greater understanding of organized labor and the electoral system. His approach is ambitious in that it develops a model of union voting behavior with theoretical foundations "in empirical theories, speculative thoughts, and historical observations," which, in combination, he uses to find how union members and their families vote and how we can explain their behavior. The result of this efforts is a multilevel analysis that is theoretically interesting, analytically crisp, and well written.

In contrast to much of the field's literature, the author finds, by way of an interesting reformulation of CPS data, that socially mobile union members from 1948 to 1964 tended to vote more Democratic than union members in general, postulating that the experiences that are linked to occupational mobility somehow impel union members toward a closer relationship with the Democratic party. His explanation of this finding incorporates the notion of "relative deprivation" and the idea that greater social mobility for unionists generates *more* rather than fewer political demands and a tighter link to the party more receptive to those demands.

Along the way Jong Oh Ra examines critically the claim that the affluence produced by the extension of the New Deal has weakened support of the Democrats among union members. He sees the drop in labor rank-and-file support for the Democratic candidates in 1968 and 1972 as short-run aberrations in a still close relationship, explaining the defections of unionists in these two elections as the result of elements specific to the candidacies of Humphrey and McGovern rather than the weakening of durable ties to the Democratic party itself. While I agree that the relationship of labor union voters to the Democratic party is much stronger than recent literature has pictured it to be—Carter did receive 63 percent of union votes in 1976—John Oh Ra tends to deal inadequately with the very role of labor unionists and their leaders in producing those Democratic candidates in 1968 and 1972. If one chooses to use variations in party candidates as a means of explaining voting behavior, one cannot, as this author does, omit discussion of labor's role in the very process of candidate selection itself.

Several other criticisms seem justified. In the effort to create a broad and scholarly picture, Jong Oh Ra does not adequately balance his critical analysis of the existing literature. As a result, one finds the book overemphasizing events, responses, and analyses before 1965, skewing an otherwise admirable historical approach to labor union voting by paying too little regard to the literature of the last ten years or so. "Recent" data quoted in the book (p. 79) turns out to have been written in 1963. Literature produced in the 1970s examining the impact of suburbanization and racial hostilities on labor union voting is not dealt with as critically as the pre-1970 literature.

In addition, while rightfully attacking the totality of the claim that the foundation of the New Deal realignment has evaporated, Jong Oh Ra tends to underplay the relative declines that have actually occurred in popular (and union) support for specific policies that were once heavily supported only a decade ago. While it is important to correct the premature "burial" of important elements of the New Deal realignment, recent research clearly identifies important changes in the relationship of voters to new and old governmental policies and programs. Jong Oh Ra's important insights into the durability of links between labor union voters and the Democratic party should not be lost because some overstatement in the book offers an easier target for criticism than do his more substantive contributions.

Overall, *Labor at the Polls* is an intriguing work because of the variety of probes, methods, and analyses that its author brings to bear on his subject. Labor union voting patterns and their impact on electoral politics have been poorly treated in the literature, particularly where quantitative analysis is concerned. Jong Oh Ra has made an important linkage between levels of analysis, and by reformulating certain important questions, this book should stimulate more productive work in the field. Along the way the author has also displayed a mind of superior range, originality, and incisiveness.

RICHARD L. RUBIN

Swarthmore College

The Politics of Efficiency: Municipal Administration and Reform in America: 1880–1920. By Martin J. Schiesl. (Berkeley: University of California Press, 1977. Pp. ix + 259. $11.75.)

Was it or wasn't it? Did they or didn't they? Were they or weren't they? It is these perennial questions about reform and machine politics in America's cities during the period from 1880 to 1920 that preoccupies historian Martin J. Schiesl. The questions are of course familiar. Was the municipal reform movement merely the manifestation of the narrow self-interested politics of an emerging upper-middle-class, urban entrepreneurial elite? Did the reformers' preoccupation with efficient administration blind them to the needs of the urban working class and poor? Were the oft-defeated reformers "morning glories" which withered in the afternoon and passed unnoticed from the scene in the evening?

Following a wide-ranging survey of four decades of American urban history, Schiesl would answer each of these questions (had he formulated them at the outset) in the negative. In his final paragraph, he sums up what seems to be the book's main argument:

> In an era of boss politics a reform administration, despite its short life, recovered a large amount of responsible civic management, instituted new forms of political leadership, and extended the welfare functions of the city. When the machine regained control of the government, those accomplishments were not lost; the people had been shown higher standards of public service which the boss wisely chose not to ignore (p. 197).

The territory that Schiesl explores is not untraveled. Indeed, mention of the issues causes names like Plunkett, Merton, Banfield, Wilson and Holli to spring to mind. In devoting a chapter to each of the major ideas and structural mechanisms that preoccupied urban reformers, Schiesl makes two contributions to our understanding of the politics of urban reform. First, because he ranges much more widely in the history of this era than any of his predecessors—from Boston to Berkeley, and Manistee (Michigan) to Galveston—Schiesl provides his readers with many fascinating and informative historical insights. To illustrate: how many students of intergovernmental relations or urban politics are aware of the important role played in the first decade of the twentieth century by the U.S. Census Bureau (under the guise of rationalizing data collection) in promoting urban reform and the broadening of municipal social services (shades of community action and model cities)? Second, Schiesl provides a new answer to the old debate over whether machine or reform politicians more effectively promoted democratic values and the well-being of the masses in urban America. He suggests that neither camp was without its internal contradictions and the synthesis that resulted from the competition between the two produced the best of all possible worlds. While the Panglossian quality of this analysis may initially provoke a few modest snickers, the more one reflects on Schiesl's analysis, the more persuasive it becomes.

While its central argument may be both fairly novel and persuasive, this is not a volume without fault. To begin with, it is by no means clear that my perception of the author's central theme corresponds with his perception. Indeed upon occasion it is difficult to be certain of just what is the author's central point. What is clear however is that creative writing is not his forte. Granted, there are few budding Hemingways (or even Grant McConnells, to think of someone who writes in a similar mode but with elegance) roaming the academic ranges these days. Nevertheless, someone at the University of California Press should be required to turn in his or her blue pencil for failing to make this book read more crisply. The total absence of chapter subheadings represents exhibit number one in any argument for such stylistic aids. Moreover, even with them, the most fervent student of the topic would not be likely to argue that the book represents a model of either clarity or liveliness.

Of greater seriousness is Schiesl's apparent inclination to sometimes mistake a quote for a fact. At times, it appears that he suffers from the same malady that many of us suffered from prior to Nixon—an absence of skepticism about the public pronouncements of public people. Particularly in the early chapters there is a maddening tendency to attempt to depict the reality of urban life and politics at the turn of the century by stringing together a series of quotes from various personages. The fact that from time to time there appear to be implicit contradictions among those whom he cites to make a particular point does not seem to faze the author. Indeed, in a few instances, a subsequent quotation from a particular individual serves to subtly contradict the point which the individual's earlier cited words were assumed to have demonstrated (see, for example, the comments of E. L. Godkin to be found on pp. 8 and 23). Nevertheless, students of urban politics do need to be aware of this book and the author's argument. Once figured out, it does prove provocative.

ALLAN ROSENBAUM

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Charting the Future: The Supreme Court Responds to a Changing Society, 1890–1920. By John E. Semonche. (Westport, Conn.: Greenwood Press, 1978. Pp. xiii + 470. $25.00.)

This is an odd book. In his preface John Semonche suggests that the role of the Supreme Court from 1890 to 1920 has never been put in its appropriate conceptual context, indicates—correctly, in my judgment—that many commentators have applied simplistic criteria to its work, and intimates that he has found the master key. Unfortunately, he left it in the latch: the next 417 pages constitute an inch-by-inch trip through the *U.S. Reports* for the period.

This sort of exercise is not without merit *provided* it is guided by some thematic compass. Twenty-three years ago, in a burst of youthful enthusiasm, I read all the Court's decisions during the tenure of Justice Frank Murphy. But I did so with an a priori hypothesis which—not surprisingly perhaps—was triumphantly sustained. Uncharitable critics hinted I had strapped the poor justice to a bed of Procrustes, but whatever the views on the merits of my work, no one accused me of aimlessness. Semonche has taken the other route: with unnerving industry he had catalogued every tree in a judicial forest planted in a 30-year period.

Admittedly, he has done this task with care, although on occasion there is a distinctly ahistorical flavor. He writes, for example, of the country's "long devotion to the separation of powers concept" (p. 428) though the only early cases in which this devotion surfaced as part of the *ratio decidendi* were *Cooley* v. *Board of Wardens* (1851) and *Murray's Lessee* v. *Hoboken Land Co.* (1855). It was apparently Justice Benjamin Curtis' private hobby and with him vanished from the precedental scene for half a century. Semonche also considers the delegation of power to the states to regulate "demon rum" a constitutional innovation; yet in 1803 Congress (2 *Stat.* 205) prohibited the importation of slaves into states whose laws prohibited slavery.

Probably my most significant criticism of Semonche's vacuum-packed approach arises from his handling, or failure to handle, the "Chinese syndrome": the Supreme Court's surprisingly willingness to defend Chinese immigrants, overwhelmingly alien, from the police power. To make a long story—brilliantly limned by Howard Jay Graham in "Justice Field and the Fourteenth Amendment"—short, the status of the Chinese in California became a litmus test of the power of the railroad magnates in their struggle with the Populists, who assimilated racism into their assault on imported scabs.

The poor Chinese, willy-nilly, became pets of the federal courts operating under "Ninth Circuit Law": the bottom line was that Chinese aliens on the West Coast had freedoms denied to black citizens in, say, Alabama. Justice Brewer, Field's nephew, and other judges continued this tradition at the national level. All in all, Semonche would have profited from reading Graham's *Everyman's Constitution.*

There are some minor nits to pick with Semonche's interpretation of various cases: I would suggest the importance of the *Northern Securities Case* was the appearance of five Justices in support of the "rule of reason" (Brewer and the four dissenters). And the Court's decision in *Hammer* v. *Dagenhart* (1918) could have usefully been compared with *U.S.* v. *American Tobacco Co.* (1911): how did a ban on interstate shipment of goods produced by child labor (bad) differ logically from one on goods produced by "trusts" (valid)? However, we both love the first Justice John Marshall Harlan, a *mensch.*

Finally, after the long march, we reach the "Epilogue" in which Semonche returns to his rationale for all this labor. He explodes some "myths" which, with due modesty, I thought I exploded in the *University of Chicago Law Review* in 1963. (Maybe they have since been revived.) He feels the Court went through a time of troubles intact; true enough: nine justices went in and another nine came out. But at the analytical level we end where we began, with a chart but no compass readings.

JOHN P. ROCHE

The Fletcher School of Law and Diplomacy

Bakke, DeFunis, and Minority Admissions: The Quest for Equal Opportunity. By Allan P. Sindler. (New York: Longman, 1978. Pp. xi + 358. $12.50, cloth; $5.95, paper.)

Few current political and legal issues are as emotionally charged as the subject of "reverse discrimination." On one side are those who believe that affirmative action and preferential treatment are forms of benign discrimination essential to compensate for past evils. On the other side are those who argue that preferential treatment for minorities violates *Brown* v. *Board of Education,* Title VI of the Civil Rights Act of 1964, and the equal protection clause of the Constitution.

Allan Sindler's book contains an excellent analysis of reverse discrimination questions posed in *DeFunis* v. *Odegaard* (1974) and *Regents of the University of California* v. *Bakke* (1978). Less emphasis is naturally placed on *DeFunis* than *Bakke,* for in *DeFunis* the Burger Court ruled that the case had become moot. Nevertheless, similarities and differences between *DeFunis* and *Bakke* are capably addressed throughout the book.

Sindler presents a detailed account of how the Bakke case arose, moved through the California courts, was appealed to the Supreme Court, and developed into a major test case. At the Supreme Court level, Sindler's focus is on the fragmented Court that, by 5 to 4 votes, ruled (1) that the quota program of the University of California at Davis Medical School was illegal, (2) that race could nevertheless be one consideration weighed in the admissions process, and (3) that Bakke should be admitted to the medical school. Unfortunately, the book fails to suggest timely recommendations on how reverse discrimination questions should be resolved in future litigation.

Despite its high quality, Sindler's book fails to convince the reader that he is a truly dispassionate observer. Underneath what appears to be an objective study are signs of a pro-Bakke bias. First, of all the opinions written in the Bakke case, only Justice Brennan's anti-Bakke stance comes under direct fire. For example, Sindler characterizes the Brennan position as tautological, incomplete, misleading, fallacious, as an "attempt to settle the matter by judicial fiat," and as seeking to "reconcile racial preferences and no reverse discrimination by defining reverse discrimination out of existence" (pp. 303–07).

As a second example, Sindler writes:

> Since the granting of race preferences in any competitive situation necessarily involved some degree of reverse discrimination, a significant judicial role is required. Moreover, the argument's premise—that one could confidently and easily distinguish between "good" and "bad" racial discrimination—was unsound (p. 192).

But contrary arguments abound. The quest for equal educational opportunity must go beyond the judiciary. The Congress, the White House, and executive branch agencies all have a legal responsibility to promote affirmative action. Obviously some non-minorities are adversely affected by preferential treatment. However, non-minorities are still far ahead in terms of education, employment, and housing. The fact is that Bakke and DeFunis had multiple career opportunities aside from attending medical or law school. Minorities rarely have that many chances. Before we can approach the ideal of equal opportunity based on merit, therefore, steps must be taken to ensure equal footing. "Good" racial discrimination may thus be defined as the amount of affirmative action necessary to guarantee that all competitors are evenly aligned at the starting gate and that no one is slowed down by the weight of past discrimination.

Third, Sindler's pro-Bakke view is seen in the fact that he discards affirmative action employment cases, like *Griggs* v. *Duke Power Company*

(1971), as irrelevant to affirmative action in education (pp. 180–88). *Griggs* held that if employment requirements which were not job-related had a disproportionate adverse impact on the hiring and promotion of minorities, that that effect—not the requirement's intent—sufficed to prove discrimination. Yet Sindler rejects the effects test even though Title VI was prominently involved in *Bakke.* Essentially, then, Sindler accepts the virtually impossible task of proving intent, not effect, in affirmative action cases.

Fourth, Sindler seems to fear the length of time necessary for affirmative action programs to achieve equal opportunity (p. 263). He also argues that over time preferential treatment will "stigmatize the favored groups" (p. 269). Both arguments are questionable. It may take more than one generation to achieve equal educational opportunity, but affirmative action is not an unending process. And although feelings of stigma will occasionally occur, most minorities will probably feel that affirmative action was necessary to assure them the opportunity for a good education.

As a final comment, Sindler concludes that *Bakke* is not a "landmark" decision and presumably will have relatively little impact (pp. 294, 301). However, this may be a premature judgment. Even the conservative Burger Court is likely to use *Bakke* to continue to support affirmative action programs. Furthermore, a future liberal Court could construe *Bakke* to advance significantly preferential treatment in areas outside education. While perhaps not the most important civil rights decision since *Brown, Bakke* may ultimately qualify as a "landmark" decision and have a lasting impact on minority rights.

CHARLES M. LAMB

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The Limits of Technocratic Politics. By Jeffrey D. Straussman. (New Brunswick, N.J.: Transaction Books, 1978. Pp. xii + 164. $14.95.)

The Limits of Technocratic Politics, which began as a dissertation, is a readable summary of largely familiar material, combined with sensible (and again familiar) commentary. Technocratic politics is the term Jeffrey Straussman uses to cover the continuing use (and misuse) of various analytic and decision-making techniques and tools. In general, he is concerned with the policy and political roles of experts—and their relations with more responsible (or higher status) decision makers. In particular, he devotes successive brief chapters to examining the rise and decline of the new (early sixties) economics, the PPBS (Planning, Programming Budgeting System), the social indicators movement, and the futures movement. Each chapter is a handy summary of major developments and each is well documented.

For the professional who followed these movements as they unfolded there is not much new. The preface by Bertram Gross says, "This book is the first empirical study of major elite movements in the technology of public planning: macroeconomics, system budgeting, social indicators, and futurology." But, in fact, the book is not particularly novel or empirical. It is a good, though brief, literature review in large part—and will be useful to students and others new to these fields. Observers who walked into the policy theatre while ZBB (Zero-Based Budgeting) was showing, or MBO (Management by Objectives), for that matter, may want to catch up by reading this book. In discussing the role of analysts Straussman points to legitimation, the focusing of issues, the selection of targets, and tactical guidance. Readers interested in further exploring the roles of analysts should by all means peruse Arnold Meltsner's book, *Policy Analysts in the Bureaucracy*—a book at once more empirical and more thorough. And, of course, one can find numerous individual volumes on each of the subjects to which Straussman devotes his individual chapters.

JAMES W. DAVIS

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Political Control of the Economy. By Edward R. Tufte. (Princeton, N.J.: Princeton University Press, 1978. Pp. xi + 168. $10.00.)

For those concerned about the lack of elite responsiveness to electoral processes, this is an optimistic book. Contrarily, for those concerned about rationality in the development of macroeconomic policy, this is a pessimistic book. Optimists, unite! You have nothing to lose but the inconvenience of modest, and, occasionally, not-so-modest economic booms and busts.

Edward R. Tufte has authored a remarkable study, one that is provocative and concise, sometimes brazen in its theoretical assumptions and assertions, laced throughout with elegance and wit. Amid the vast outpouring of contemporary social science, *Political Control of the Economy* truly is a book for all seasons, important to every scholar interested in the relationship of elections, government, and public policy.

The major theme, *contra* Marx and mildly ironic, is best stated by the author: "As goes politics, so goes economic policy and performance. This is the case because as goes economic performance, so goes the election" (p. 137). Tufte marshals an impressive array of new and secondary evidence, relying primairly but not exclusively on American data from the past three decades, to explore these ideas. The basic argument is not original (political economists and a few political scientists have been tinkering with these notions for years) but the author organizes his data and analysis in such refreshing fashion that even mainstream political science will be hard put to ignore the message.

In the United States, and increasingly in Western Europe, public officeholders have engineered an electoral-economic cycle geared to maximizing the reelection prospects of the party in power. For example, from 1948 to 1976 the U.S. electoral-economic cycle has consisted of (1) a two-year cycle in the growth of real disposable income per capita which accelerates in even-numbered (election years) and decelerates in odd-numbered (nonelection) years; and (2) a four-year presidential cycle in the rate of unemployment, with a decline occurring in (surprise) election-reelection years, and an increase beginning 12 to 18 months following the presidential election. Moreover, the greater the electoral stakes, as in attempts by incumbents to regain office, the greater the economic stimulation. The Eisenhower administration is the only postwar exception, a point to which I will return. The Nixon administration, in this as in most things, far outran all others in its enthusiasm to induce short-run economic gains. Tufte shows that the typical fourth quarter of a presidential election year is characterized, not only by a decline in unemployment and an increase in real disposable income, but also (a) dramatic increases in social security and veterans' benefits, usually and conveniently delivered to recipients in October and which account for almost 50 percent of all federal transfer payments; (b) substantial increases in the supply of money through various federally induced activities; and (c) sudden unexplainable bursts of federal grant deliveries to states and localities. American presidents seem to follow a variation on Mark Green's Golden Rule: he who distributes the most gold before election day, rules.

Tufte then makes clear that such activities are not confined to the United States but increasingly characterize the political economies of all industrialized democracies. Using data from the seven largest, Tufte notes that GNP growth rate during election years, for example, is nearly double the rate in nonelection years. Election-year expansion in the U.S. appears to spill over to other countries, too, since the "growth rates in real GNP in *other* countries are nearly double in the years with U.S. presidential elections compared to years without elections in the U.S. or the country itself" (p. 66). What accounts for this?

Tufte provides a suggestive piece of empirical detective work. Since 1971, the major European democracies plus Japan have increasingly followed the American electoral calendar, holding elections in even rather than odd-numbered years. "With the synchronization of election calendars in large capitalist democracies, we have a recipe for an international boom and bust cycle" (p. 69). Tufte does not, however, have sufficient evidence to convince one that it is the electoral cycle, and not simply the increasing interdependence of a world economy, that is responsible for these effects.

The remaining parts of the book are concerned with examining the economic determinants of voting (a summary and slight expansion of Tufte's earlier published work) and specifying party and class differences in government economic policy, again focusing primarily on the United States but including a discussion of comparative data from Western Europe and Japan. Most of the party/policy data are taken from the recent work of Douglas Hibbs, Jr., and Tufte does not improve on Hibbs' earlier analysis. Nor, I might add, does he discuss any of the criticism that has been directed at this line of inquiry. Tufte does incorporate some imaginative content analysis of the *Economic Reports of the President* and the *Annual Report of the Council of Economic Advisers.* Consistently with the work of Gerald Pomper, John Kessel, and others who have examined party-government documents, he shows that the macroeconomic policy preferences of decision makers diverge sharply, depending on whether a "left-liberal" or "right-conservative" party is seeking and/or holding office. This generalization holds for both American and West European political systems. Given the apparent uniform effect of the election calendar, how "sharp" is party divergence? The question is hedged for the United States and Hibbs' conclusions are repeated for Western Europe. Tufte is quite sensitive to methodological deficiencies here (involving time-series analysis and within-country variation) as well as the very powerful cultural and historical differences between societies. Still, his conclusion is worth quoting (p. 104):

> The single most important determinant of variations in macroeconomic performance from one industrialized democracy to another is the location on the left-right spectrum of the governing political party. Party platforms and political ideology set priorities and help decide policy. The consequence is that the governing political party is very much responsible for macroeconomic outcomes–unemployment rates, income equalization, inflation rates, and the size and rate of expansion of government.

Tufte's and other studies seem persuasive on this argument, so even closet responsible-party theorists and election enthusiasts can feel vindicated. However, the more fundamental question is: how important are elections and parties compared to *other*, largely nonelectoral, factors?

For a volume written by one of the profession's most sophisticated methodologists, *Political Control of the Economy* is curiously indifferent to the actual magnitude of policy change in some parts of the analysis, while greatly overestimating magnitude in others. Consider Tufte's comparative data on income equalization (at p. 95). Information about the share of national income held by the top 20 percent of income earners, before and after taxes, is presented for 11 countries. Predictably, those societies where the Left has governed the longest (Sweden) have the highest rates of income equalization based on the impact of direct taxation. But what is more striking, even if one accepts Tufte's measure as a valid index of government equalization activity, is how small the actual shift has been, even in Sweden. The pre-tax share of wealth held by the upper fifth in Sweden is 40.5 percent; the post-tax share, 37 percent, a net "redistributional" shift of 3.5 percent. Comparable figures for the United States are 44.8 percent before taxes, 42.9 after, a shift of 1.9 percent. Since this is the only income equalization data examined in the book, one can be forgiven for concluding, as Mark Twain did about his own death, that the impact of the Swedish and other left-liberal parties are greatly exaggerated in Tufte's analysis. Obviously, there are large and more important economic differences between countries like Sweden and the United States in terms of income and service distribution, but nowhere in this book are such differences examined. Indeed, the problem of income distribution, a problem one might consider central in a study of political control of economic policy, receives scant attention for any society, including the United States. Why? Since Tufte largely excludes nonelectoral factors, his "model," greatly truncated, inevitably looks like this: Parties → Elections → Economic Policy. One need not be a Marxist (Neo- or otherwise) to posit a somewhat different causal sequence: Economic Structure → Parties → Elections → Economic Policy. The author, perhaps intentionally, avoids dealing with this issue.

Or consider the heavy emphasis placed on the *rate* of changes in real disposable income. The median growth rate in real disposable income when an incumbent American president is seeking reelection (Tufte's maximum-impact hypothesis) is +3.3 percent; in other years, +1.7 percent. For a family of four, with $15,000 annual income and who are at the median, the actual dollar gain during the election year is a paltry $240. I realize that disposable income is a summary economic measure, that the psychology of "good" versus "bad" times is more important than actual amounts, and so forth. Tufte nevertheless loads considerable theoretical baggage on such small changes. Further, he ignores the fact that the direction of change in real disposable income has moved up, even though the rate has been comparatively low, in 13 of 15 *non*election years, compared to 11 of 15 years in which elections were held. His conclusions depend substantially on excluding the Eisenhower administration, when real disposable income declined during two out of eight years, and focusing on the rate rather than direction of change. (I am indebted to William R. Keech for this observation.) In any event, structural characteristics of the American economy seem much more important in determining macroeconomic outcomes than does the rhythm of elections or the decisions of public officeholders, particularly as these factors bear on the harsh and enduring inequalities of American life. Tufte concedes some of this in a final chapter, but much of the book is written as if structural aspects of the economy, and their specific relationship to the political system, were too obvious to be addressed.

The volume's final chapter is devoted to examining policy and political implications. In the absence of elections, the Democratic party, and a few (overly) eager Republicans like Richard M. Nixon and George P. Shultz, Tufte reasons, macroeconomic policy in the U.S. would have developed along lines acceptable to the William McChesney Martins and James M. Buchanans of economic life. So the electoral process has helped keep the economy growing, has provided incentives to move large government transfer payments to those who are organized, and has maintained unemployment at levels low enough to prevent widespread working-class discontent. Like many of us, Tufte sees this as a plus. On the other hand, political economists like Buchanan increasingly have legitimate complaints. Tufte forcefully argues there are considerable disadvantages in election-induced expansion followed by the inevitable contraction, when he writes that it "breeds a lurching, stop-and-go economy the world over. Governments fool around with transfer payments, making an election-year prank out of the social security system and the pay-roll tax. There is a bias toward policies with immediate, highly visible benefits, and deferred, hidden costs; . . . special interests induce coalition-building politicians to impose small costs on the many to achieve large benefits for the few. The result is economic instability and inefficiency" (p. 143). He concludes by suggesting some sensible remedies that would retain the representational advantages of the electoral-economic cycle but do not depend on reaching one of the Holy Grails of modern economics, some optimal point on a Phillips Curve.

The historical period covered ends in 1976. After four years of stagflation under both a Republican and Democratic administration, increasing concern over environmental destruction and the politics of scarcity, and an election year fast approaching, one wonders if Tufte's predictions about economic expansion will be sustained in the late 1970s and early 1980s. Inflation is a problem that even liberal Democrats must confront, if for no other reason than the electoral necessity of holding middle-class support.

JEFF FISHEL

American University

Lobbying the Corporation: Citizen Challenges to Business Authority. By David Vogel. (New York: Basic Books, 1979. Pp. xi + 270. $14.95.)

David Vogel has written an extremely interesting study of citizen groups and their corporate adversaries. *Lobbying the Corporation* is a comprehensive analysis of the advocacy aimed at making business more accountable to the public.

The common goal of corporate accountability groups is to convince business people that citizens play a legitimate role in trying to shape corporate policy. Corporate decisions affect people outside the corporation; citizens must thus do what they can to influence the corporation. Business decisions are viewed by these interest groups as moral and political choices. Corporate directors and managers have, in turn, conceded little. Their general perspective is that a corporation is autonomous, and that they have no obligation to be responsive to the demands of such groups.

Vogel traces the rise of corporate responsibility groups to the civil rights and antiwar movements. During the Vietnam War many Americans began to give serious thought to the role of corporations in American political life. Dow Chemical, Honeywell, and other corporations found themselves having to defend their "right" to pursue profitable, war-related activities. Antiwar groups were effective in raising the issue of corporate accountability, however, and further lobbying followed the war's end.

The largest portion of Vogel's study is devoted to examining specific campaigns by citizen groups to affect corporate behavior. He looks at three broad areas: shareholder proxy wars, corporate disclosure requirements, and corporate responsibility abroad. His data is comprised largely of case studies of numerous lobbying efforts. These short histories are well-documented accounts of various groups and their attempts to influence individual corporations. Included among these case studies are profiles of Campaign GM, the Angola-Gulf Oil controversy, and the struggle between citizen groups and Kodak. Vogel devotes a great deal of space to the conflict between citizen groups and corporations doing business in South Africa, clearly the most significant corporate responsibility issue of the past few years.

In his final chapter Vogel addresses the question as to whether these groups have had any impact—the bottom line, so to speak. He concludes that the direct influence of citizen groups on corporate decision making has been rather marginal. At the same time, though, Vogel argues that their influence is impressive in a more indirect way. Although corporations do not generally buckle under to hostile challenges to their authority, they now find themselves making decisions in a far different environment from the one that existed prior to the corporate accountability movement. Citizen groups have "politicized" the corporation; executives must make their decisions in light of the potential public scrutiny of their actions. Still, Vogel finds the successes of citizen groups far too limited, and he makes the case that they are unlikely to become more influential in the future. His solution to making the corporation more responsible is the traditional liberal approach: more governmental intervention.

A disappointing aspect of this book is that Vogel makes little effort to integrate his study into the larger literature on interest groups. He does not use his data to question prevailing generalizations or theories concerning interest group organization, lobbying, or the role of such groups in a democracy. Nevertheless, Vogel's study adds immeasurably to what we know about citizen groups in American politics.

Overall, *Lobbying the Corporation* is a book well worth reading. It is an excellent work that forces one to consider the political and moral ramifications of corporate activity. Citizen groups have not yet been able to turn American corporations into the type of "citizens" that many of us might like. But their success is that they've made business people increasingly aware of the ethical dimensions of their actions. At least that's a start.

JEFFREY M. BERRY

Tufts University

Criminal Lawyers: An Endangered Species. By Paul B. Wice. (Beverly Hills, Calif.: Sage Publications, 1978. Pp. 231. $15.00, cloth; $7.95, paper.)

Criminal Lawyers is based on 180 interviews conducted with private practitioners in nine cities. To his credit, Paul Wice is frank in admitting that his approach was less than rigorous; he labels his methodology as one of "sociological journalism" (p. 19). He is also frank–though cryptic–in informing the reader that "it is estimated that 45 percent is strongly documented evidence, 40 percent strong belief based on incomplete evidence, and 15 percent based on insight and intuition" (p. 18). This confession suggests caution in reading this work. One is never quite sure from whence spring some of the author's contentions. Does argument A emerge from the 45 percent, the 40 percent, or the 15 percent category? Is generalization B a product of the author's insight or is it the modal response of the attorneys? Wice does present dissenting views to some of the themes he discusses, but often the reader is simply not sure about the representativeness of a particular argument.

The book is primarily concerned with describing the background, attitudes, working conditions, and work styles of the private criminal attorney. Though much of the material covers familiar terrain, and some of the analysis of the interview data is superficial, there are a number of interesting points tucked away in *Criminal Lawyers*. Especially significant and interesting are the discussions of: (1) the consequences of the increased threat of ineffective assistance of counsel suits on the behavior of defense attorneys (pp. 148–49, 165); (2) the Washington D.C. subsidy program for middle-class defendants who cannot afford to retain counsel on their own (pp. 208–10); (3) the retention of criminal attorneys by large firms for white-collar crime, apparently a growth industry (p. 92); (4) the dynamics of fee-setting by the private attorney (pp. 108–15). In each of these areas (and in several others that space constraints preclude mentioning) Wice touches upon some new material, and whets our appetites to learn more about the issues at hand. Unfortunately, the "everything you always wanted to know about criminal lawyers" approach seems to lead him away from delving into these subjects in any detail.

A second area of strength of the book is its attempt at some comparative analysis of defense attorney behavior. Wice's thumbnail sketches of the nine communities are well done, and his delineation of the variables shaping their differences is an interesting catalogue of institutional, legal, geographic, political and social factors.

Criminal Lawyers, however, also suffers from several weaknesses. First, the "endangered species" theme is not well developed. What was intended here, I believe, was an argument that with the advent of the public defender system, the private counsel system of representation became "endangered." Yet with one exception toward the end of the book in which he reports the gripes of some attorneys about judges who assign public defenders to indigents who could afford counsel, and with the possible exception of opting for assigned counsel in lieu of public defenders, Wice never really demonstrates that the public defenders themselves are taking business away from the private attorneys, nor systematically addresses the question of how defendants who can afford counsel can opt for a defender. Wice reports that a consequence of diversion programs for the private attorney is that "their potential clients were now going to the public defender because his office could just as easily get them probation as a private attorney, and they could save a few thousand dollars in the process" (p. 115). But defendants cannot simply choose one over the other. The requirements for obtaining public defenders may be lax, and tightening them might make sense. But there is nothing inherent in a scheme of representation for the indigent that renders paid counsel obsolete or endangered. Relatedly, the author seems to mourn this perceived demise of the private attorney for reasons that he never makes very clear. He tells us that as his interviews progressed he became "more and more sympathetic to their plight" (p. 18), and the book is coated with a veneer of sympathy for "this beleaguered profession" (p. 13). Yet these are the same attorneys who are antagonistic to diversion and rehabilitation programs because they "view these reforms as deleterious to their professional existence" (p. 216), and who "not only disliked their clients, who were generally categorized as either a necessary evil or simply a 'pain in the ass,' but were also contemptuous of their colleagues" (p. 219). Maybe extinction would not be such a bad idea after all!

A second weakness of the book involves its organization and style. Chapters end abruptly without much attempt at integration of the interview material. The book is dotted with seemingly irrelevant observations about the relative attractiveness of different cities and different lifestyles (see, for example, pp. 40, 82, 102), and often it is unclear whether it is the author or the respondents who are making these observations. And since no control group of attorneys was interviewed—a weakness the author recognizes—it is difficult to determine whether the personal problems and traits attributed to the criminal attorney are unique, or whether these characterize other attorneys as well.

In sum, *Criminal Lawyers* is a wide-ranging survey of the lives and styles of the private practitioner. It is not as tightly presented and argued as one might like, and it does not develop its "endangered species" theme satisfactorily. Yet it also raises a number of interesting questions, presents some new information, touches upon a number of important issues, and provides an overview of what the private practice of criminal law is like. Given the current fashionableness of legal careers for undergraduates, some instructors might find this a useful supplementary text for an introductory legal process course. If nothing else, students will learn that they ought to give torts a second thought.

MILTON HEUMANN

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Comparative Politics

The Spanish Political System: Franco's Legacy. By E. Ramón Arango. (Boulder, Colo.: Westview Press, 1978. Pp. xi + 293. $20.00.)

There has been increasing interest in recent years in the evolution and dissolution of authoritarian governments. This is the central concern of E. Ramón Arango's study of Franco's political legacy. For Arango the origins of the Spanish Civil War, and hence of the Franco regime, lie in a late developing country's social divisions and lack of a common political culture. He attributes the longevity and apparent success of the regime to its atavistic appeal and to Franco's political skill. Civil war forged a coalition of privilege, and for nearly four decades Franco played one rightist faction against another. He gave each the necessary taste of power, but became the captive of none. Meanwhile his social programs took the edge off working-class militancy, but were not so ambitious as to bridge the cleavage on which is rule was predicated.

But if tactically adroit, Franco had little understanding and decreasing control over the new consumer society that resulted from his 1959 decision to abandon statist autarky for an increasingly unfettered capitalism. As Arango states, economic growth produced a politically restive middle class while the maldistribution of new wealth assured continuing conflict between the old haves and have-nots. Well before the generalissimo's death, the decomposition of his regime began.

These circumstances augured ill for King Juan Carlos, much of whose shaky legitimacy derived from being Franco's hand-picked successor. But Juan Carlos soon demonstrated surprising political instincts of his own and a sense of long-term political realities far superior to Franco's. Standing at the apex of Spain's structure of privilege, Juan Carlos and his prime minister—Adolfo Suarez—have maintained the integration of their country while liquidating its rightist dictatorship. Their success—especially in maintaining the army's loyalty—stands in marked contrast to the leftist republicans and socialists who dismantled Spain's previous dictatorship in 1931.

Arango tells this story well. His generally good grasp of his subject is all the more striking because he undertook little apparent original research. His sources are disproportionately English and American, and all too often recent Spanish scholarship is neglected. His chapter on the civil service, for example, is one of his strongest, but it makes no reference to Miguel Beltrán's work on bureaucratic elites. There are also the usual problems attendant upon writing contemporary history. Arango's concluding pages render his earlier detailed chapter on the National Movement and its potential role in post-Franco Spain of little more than antiquarian value.

Perhaps somewhat more importantly, he treats the Franco regime a bit more sympathetically than I am inclined to accept. Certainly most Spaniards were pleased by the extended social peace enforced by the regime, but it is excessive to state that in 1964 "that accomplishment alone would have been enough to justify Franco's rule" to his fellow countrymen (p. 209). Repression was a mainstay of the regime until its end. The shallowness of its roots have been more recently evident in ths dismal electoral record of the franquista Alianza Popular.

Doubts also exist about Franco's "economic miracle" (p. 210). His last two decades in power saw a significant increase in national wealth. But much of that wealth derived from tourism and emigrant remittances. In other words, as a result of the limited employment opportunities and low wage and price levels associated with underdevelopment, Spain became the caboose on a train pulled by the booming economies of northern Europe. Industrial growth certainly occurred during the 1960s and 1970s, but it is still an open question whether Spain could hold its own in the EEC if its bid for membership in that organization were someday to be accepted.

Finally, there are a few minor quibbles. Primo de Rivera founded the Unión Patriótica, not the Unión Popular (p. 49), and Alfonso XII died in 1885, not 1855 (p. 30). Neither large nor small reservations, however, should detract from the main point of this review. Arango has produced a useful and well-written study of twentieth-century Spanish politics and especially of the transition from authoritarian to democratic government.

JAMES RIAL

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Soviet Nationality Policies and Practices. Edited by Jeremy R. Azrael. (New York: Praeger, 1978. Pp. xi + 393. $25.00.)

Jeremy Azrael believes that nationality problems are unlikely to reach crisis proportions in the USSR, but that "these problems are neither adventitious nor recessive, and could significantly influence the future development of the Soviet system" (p. 363). This sober approach characterizes his own examination of the political, economic and military consequences of the growth of the Central Asian Soviet population and the shrinkage of the European one. It also informs almost all the other essays in this collection, outstanding among works on Soviet nationalities for its non-polemical treatment of a highly controversial topic. In contrast to the usual conference volume, this collection contains articles of 25 to 40 pages in length, based on original research in primary sources, with extensive documentation and, for the most part, the effective application of quantitative techniques of analysis.

Articles by S. Frederick Starr on tsarist imperial policy, by Helene Carrere d'Encausse on the evolution of Soviet nationality policy, and by John Armstrong on the "mobilized diaspora" of the Baltic Germans in the tsarist empire set the historical background. Starr's excellent treatment is a rare analysis of tsarist imperialism and colonialism; one can see how, at least in this instance, Bolshevik policy departed significantly from Russian precedents. The relevance of Armstrong's highly detailed study to Soviet policies is not immediately apparent, but those familiar with his other work on "mobilized diasporas" will be able to draw the proper inferences.

Two articles, one by Yaroslav Bilinsky and the other by Charles Fairbanks, bridge the historical and contemporary. Bilinsky's essay on the Ukrainian "national Communists" Mykola Skrypnyk, active in the 1920s, and the more contemporary Petro Shelest, is informed by the writer's immense expertise on the Ukrainian Communist party and offers impressive evidence that Shelest did, indeed, follow in Skrypnyk's tradition. Shelest's well-publicized hawkishness in foreign policy—he pushed for the invasion of Czechoslovakia and against Brezhnev's policy of detente—is seen as a protective device to shield him from being accused of a "rightist, pronationalist deviation in both domestic and foreign policy: it was 'preventive hawkishness' " (p. 126). The weakness of the essay lies in its labored comparisons of Skrypnyk and Shelest and in its ultimate failure to link the two, though this does not vitiate the argument for the "remarkable persistence of Ukrainian national Communism."

Probing deeper than other scholars, Fairbanks makes a plausible argument that Lavrenti Beria tried to use the nationality issues "as his principal weapon in an attempt to become ruler of the USSR" (p. 144). Fairbanks' persuasive, careful textual analysis shows Beria daring to oppose Stalin's "anticosmopolitan campaign" in 1949, and going against the leader's line on nationalities in a major address at the Nineteenth Party Congress (1952). Fairbanks argues that Stalin chose the nationalities issue around which to arrange his succession, while Beria tried to compensate for his lack of support within the party by appealing to the minority cadres. The fact that Beria did not succeed does not, in Fairbanks' view, prove that the nationality issue is irrelevant; Beria failed because of the East German revolt and the outcome of the "palace conspiracy."

While survey data on ethnic attitudes in the USSR are sparse, aggregate data on language use, residential patterns, and ethnic affiliation are relatively plentiful, though often tantalizingly incomplete. Jonathan Pool, Brian Silver and Barbara Anderson demonstrate brilliantly how aggregate data can be mined in order to discover trends among the ethnic groups of the USSR. All three come to roughly similar conclusions, though their foci are different: Pool argues that while Russian will not displace the 15 Republic languages, the other languages are likely to fade away; Silver shows that Russian may be acquired as a second language by many, but the next step—adopting Russian as a "mother tongue"—is a very long one indeed, taken by relatively few; Anderson shows that the peoples of the autonomous republics within the RSFSR are changing their nationality designations to "Russian." Alexandre Benningsen's essay on the "Muslim conservative opposition" takes on added significance with the increasingly visible role of militant Islam in domestic and foreign politics in a variety of countries.

To me the least persuasive essay is Steven Burg's on the "calculus" of Soviet anti-Semitism. Though he makes some factual errors (e.g., that Islamic society was free of anti-Jewish prejudices and pogroms), he sets forth an interesting thesis—that official anti-Semitism is a rationally calculated means "by which the Soviet leadership seeks to achieve its substantive policy goals" (p. 189). Burg then strains to fit available data into this argument, and to ignore that which does not conform to the 16 axioms, two diagrams, and one calculus formula he earnestly develops. Burg's "calculus" has the leadership moving against anti-Semitism when this is in their "rational" self-interest. He can only cite one passing reference in a single *Pravda* editorial as evidence of "moving against antisemitism." Likewise, his argument that the anti-Zionist campaign after the 1967 war hurt Soviet production, research and development (because of Jewish alienation) is completely unsubstantiated, as his conclusion that "this led the soviet leadership to abandon antisemitism as a tool of policy late in 1969" (p. 208). Burg's thesis is serious and plausible, but in his overenthusiastic attempt at "scientific rigor," he becomes the willing prisoner of his own constricting formulae.

This volume is too diverse in its subjects to offer a synthesis of "Soviet nationality policies and practices," nor was that the aim of the editor, but it certainly offers fresh insights into some of them.

ZVI GITELMAN

University of Michigan

Faction Politics: Political Parties and Factionalism in Comparative Perspective. Edited by Frank P. Belloni and Dennis C. Beller. (Santa Barbara, Calif.: American Bibliographical Center–Clio Press, 1978. Pp. vi + 471. $19.75.)

What is a faction? In this stimulating set of studies, you may take your pick, Are factions organized, relatively stable, and clearly identified intra-party groups, influencing the division of power and the making of policy on the national level? By and large, the investigations by Haruhiro Fukui, Frank Belloni, Myron Aronoff, and Arthur Cyr of factionalism in Japanese, Italian, Israeli and British parties, respectively, take this approach. From them we may glean such useful generalizations as these: intra-party factions often have their source in the groups which originally combined to form the party; faction members' apparent preoccupation with ideology and leadership are frequently mere covers for the pursuit of power and the other spoils of office; party rules devised to accommodate the presence of factions have the sometimes unlooked-for effect of institutionalizing them.

Is it useful to stretch the concept of faction to include clientelistic relationships, where organization is weaker and loyalties to individual leaders are stronger, where local politics is more often the key arena and attachment to national parties can be fragile and transitory? Those who think so will find the summary of empirical findings presented in Raphael Zariski's introductory essay enlightening, and will welcome K. G. Machado's study of factionalism in the Philippines before the 1972 coup, as well as Norman Nicholson's report on how Indian village-generated factions lack the "media of exchange" and sufficiently generalized value structures to participate effectively in national decision making. Such readers will also appreciate Ronald McDonald's study of Colombian and Uruguayan factions in which the comparative method is used to such good effect.

So far, so good. But what shall we do about nations whose politics are so volatile that political groups keep bursting the bonds of parties altogether, forming cross-party liaisons, redividing and recombining with every electoral season? Bruce Campbell and Sue Ellen Charlton discuss these characteristics of French politics under the aegis of factionalism (neglecting in the process the opportunity to clarify machinations in the French center, but offering an insightful study of recent Socialist reorganization). Similarly, Christopher Mitchell and Jorge Nef believe factionalism can be evidenced by party mytosis within a single ideological movement and include divisions within "the left" and "the right" in their respective studies of Bolivia and pre-coup Chile.

Having gone this far, why stop? What else might we call faction? Peter Merkl labels nonpartisan voluntary associations which wrest control of local government *away* from parties as factions in his chapter on West German politics. Thomas Roback and Judson James find American factions in national candidate-oriented tendencies, cross-party legislative alliances, youth and women auxiliaries, extra-party reform clubs, and in the policy subcommittees of the parties' national committees. For Carl Linden factions may break the bonds not only of party but of nation: polycentrism in the worldwide Communist movement is his example. And Andrew Nathan takes us all the way through the looking glass: factions do not have to be seen in order to be analyzed; he suggests (not at all unconvincingly) that the Chinese have been acting for many years *as if* caught in the grip of factional politics.

For editors Frank Belloni and Dennis Beller, the question of "context" is interesting for classification purposes, but should not be made a basis for restricting use of the concept; factions *may* be found everywhere. For them the more interesting dimension is degree of organization, which they subdivide into factional cliques or tendencies, personal client-group factions, and institutionalized or organizational factions. Also intriguing to them are the causes of factions, categorized as societal, political or structural (a puzzling distinction, since several of the "political" factors they cite are systemic structural factors). But their chief concern is to give the concept of faction a new legitimacy and importance. They deplore what seems to them an excessive preoccupation with party and eagerly point out the capacity of factions to represent neglected interests and to contain conflict. Unfortunately, this effort to clear the tarnished name of faction leads them to minimize the evidence of their own authors (especially Aronoff, Nicholson, McDonald, Fukui, Machado, Nef and Mitchell) that such functions are performed reasonably capably by factions only so long as politics is confined to intra-elite struggles, that when modernizing impulses (including the need for coherent planning as well as the push for mass participation) are channeled into factional politics, the most likely results are oppression, immobilism, right-wing coups d'état, and/or bureaucratic circumvention of factional politics in order to *get things done*.

This is not to say the editors do not have a point. If we cannot agree that factions are so frequently benign in their effects as they suggest, nor that the study of factions can reasonably be made so all-inclusive as in this volume, we can grant the need to direct more of our attention to "organized units of competition not fixed by definitions of party." It does not seem, however, that we get much farther by calling *all* such units "factions." If that concept is to be useful, it must have its parameters like any other—the efforts by Zariski in this volume and elsewhere would seem to be on the right track, as would the Machado definition: "Factions are groups within a party which have structure and purposes distinct from the formal structure and purposes of that party."

Furthermore, and finally, what these studies suggest is not only that we need to go beyond faction (as well as beyond party) in identifying units of competition but also that we need to go beyond identifying the units to identifying the processes of change from one to the next. Under what conditions do clientelistic networks evolve into political machines? When can we expect intra-party factions to form cross-party alliances—or to separate into individual parties? Are local nonpartisan associations ever susceptible to partisan assimilation? There are the beginnings of answers to these and related questions in the studies in this volume.

KAY LAWSON

San Francisco State University

Constitutional Change in South Africa. Edited by John A. Benyon. (Pietermaritzburg: University of Natal Press, 1978. Pp. xxiv + 297. $15.00.)

In recent years there has been a marked upsurge in the debate about constitutional alternatives for South Africa. One of the hopeful signs in this vigorous debate is that it is now difficult to find any defenders of the status quo. There is a strong and broad consensus that radical constitutional reforms will be necessary, and that these should be designed in such a way as to ensure peaceful and democratic development. But here the consensus ends; profound disagreement prevails with regard to the precise form and direction of constitutional change.

Constitutional Change in South Africa represents one stage in the debate and it also offers an excellent introduction to the many issues and interests that are at stake in it. The book consists of the papers presented at the conference on "Constitutional Models and Constitutional Change in South Africa" which was held on the Pietermaritzburg campus of the University of Natal in February 1978. The conference was attended by 163 South Africans—academics, politicians, business people, journalists—representing a wide spectrum of political opinion and ethnic and cultural affiliations, although unfortunately, as the foreword points out, the views of imprisoned and "banned" individuals and outlawed organizations, which "may later prove important in any settlement," could not be represented (p. xiii). Most of the articles were contributed by the academic participants in the conference. The only exception is the chapter by D. Worrall, whose academic background is in political science and law, but who outlines and defends the constitutional proposals introduced by the National party cabinet in 1977 in his capacity as a politician and a N.P. member of parliament (pp. 127–35). At the time of the conference, the opposition parties had not yet completed the formulation of their constitutional counter-proposals. With one exception, the articles are in English. The one chapter in Afrikaans is followed by a three-page English summary (pp. 199–220).

On the whole, the analytical chapters are admirably clear and concise. Even more interesting is the lively discussion of the papers by the other participants in the conference. The editor wisely included extended summaries of their remarks in the book, and used approximately one-fifth of the available space for this purpose. The themes that are treated are the historical background of constitutional change, the proposed alternative constitutional models and prescriptions—federation, confederation, consociation, partition, and so on—and the social, economic, fiscal, demographic, and strategic consequences and implications of the various constitutional options. Because these topics are closely interrelated and because so many authors and discussants present their views, it is a pity that the book does not have an index. The editor explains (p. xxiii) that the tight publication schedule did not permit the compilation of an index, but that the lengthy introduction, which tries to summarize the main lines of argument, is intended as a partial remedy. The introduction (pp. xv–xxiv) is indeed helpful, but it is not really a satisfactory substitute.

There are two reasons why the book is of special interest and significance. First, it is indispensable for anyone who wants to follow the evolving constitutional debate in South Africa. Second, both the chapters and the discussions abound with references to constitutional and political forms and practices in other countries; hence the book can be regarded as an example of *applied* comparative politics and government.

AREND LIJPHART

University of California, San Diego

Voting, Caste, Community, Society: Explorations in Aggregate Data Analysis in India and Bangladesh. By Harry W. Blair. (New Delhi: Young Asia, 1979. Pp. xvi + 199. $12.00.)

Among the developing nations of the world, India is unique in maintaining two traditions, both of which are extremely important for scholarly analysis of social and political trends in the subcontinent. One is the unbroken tradition since 1950 of democratically holding competitive elections. The other is an equally important tradition of holding decennial censuses, dating back to 1872. The last complete census, consisting of some 1100 volumes, was taken in 1971.

A number of scholars have attempted to use census data to shed light on election data. This enterprise is marred, however, by one serious difficulty. The boundaries of the electoral constituencies and the census tracts do not coincide, thus making a match between census and electoral data nearly impossible. Several scholars have solved this problem by aggregating both the electoral and the census data to the level of the Indian district, where boundaries for both data are more likely to be similar.

Harry Blair's book is distinguished by the fact that he has managed to compare election and census data at the level of the polling booth by employing two techniques. One is a simple method of interviewing leading political candidates about the characteristics of the people in the polling station (e.g., whether a particular caste group is dominant); the other method employed is "isoplethic mapping," which consists of superimposing an electoral constituency map over a map depicting census demographic characteristics of areas (pp. 23, 36). Each constituency or electoral seat can then be appropriately coded by observing isopleths within its area.

Voting, Caste, Community, Society is a collection of essays of varying quality and analytic techniques. The book has no single theme or an overall thesis which can be easily summarized. Some of the questions raised are: the relationship between caste dominance in an electoral constituency and voting turnout, the differential impact of caste on electoral fortunes and debacles, the success and failure of the Muslim candidates and the relationship of their fortunes to Muslim concentration, the relationship between density of population, literacy, and rural development to voter turnout and party strength. Among the important findings in the book are these: that the upper castes are over-represented in the state legislature in proportion to their population; that Muslim representation has shifted from statewide basis to areas with heavy Muslim concentration; that population density is positively correlated with voting turnout; but that, contrary to expectations, rural development had a depressing impact on participation.

The essays are self-consciously methodological, including such techniques as simple correlations, multiple regressions, analysis of variance, and factor analysis. However, Blair seems to get bogged down in his methodological discussions. In the end, it is not clear which factors are most important and which least important for understanding electoral behavior in India.

The book is definitely designed for the specialist in Indian politics, one with a solid foundation in statistical analytic techniques. To make his book meaningful for the generalist, Blair could have been more comparative both in the context of what is already known about Indian political behavior as well as in the context of findings on political participation generally. Lacking such a comparative focus, the book is likely to stay on library shelves rather than be read by any sizeable section of the scholarly community.

M. LAL GOEL

University of West Florida

The Spanish Revolution: The Left and the Struggle for Power during the Civil War. By Burnett Bolloten. (Chapel Hill: University of North Carolina Press, 1979. Pp. xxv + 664. $29.00.)

The demise of the Franco regime, the restoration of monarchy, and the establishment of parliamentary democracy are recent occurrences in Spain which have attracted the attention of political observers. Given these changes, the appearance of Burnett Bolloten's comprehensive study of the Spanish Revolution is particularly timely and relevant. The study is an expanded version of the author's *Grand Camouflage*, published nearly two decades ago. Because of its wealth of documentation, detail, and insights, many specialists on Spain surely will acclaim this new work as one of the definitive histories of the Civil War.

The account traces events from 1936 to 1939. It is divided into six parts, five of which focus on the first two years and an epilogue which carries the discussion to the end of the civil war in 1939. This final chapter is to be developed into a separate volume.

The first part sketches events of the last years of the Republic up to the outbreak of civil war during July 1936. Ideological positions among individuals, groups, and parties contending for power are clearly juxtaposed so that one understands the tenuous position of the liberal republic which had prevailed since 1931. Inevitably, two Spains polarized by class and political allegiances confronted each other. Bolloten focuses here especially on the repercussions of collectivization carried out by the anarch-syndicalist and socialist movements upon artisans, small manufacturers, merchants, and peasant owners, tenant farmers, and sharecroppers.

The second part describes the rise of the Communists in the face of the emerging class struggle in the countryside and cities. Bolloten shows how the Communist party was able to embrace the disenfranchised interests of the urban and rural middle classes and defend them and their property rights in the face of the rampant collectivization drive. The Communists claimed that their struggle was on behalf of this bourgeois democratic, not the proletarian, revolution. There followed the establishment of the Seventh Congress of the Communist International in 1935.

Part 3 shows how the Communists attempted to dismantle collective farms under anarcho-syndicalist influence. However, a balance of class forces was necessary. On the one hand, a Communist alliance with the middle strata was used to undermine the strength of the extreme left. On the other, Communists attempted to gain control of the military forces.

Part 4 examines the effort to mold a regular army from revolutionary militia. Essential was the formation of the celebrated Fifth Regiment under Communist leadership, the presence of Soviet arms and military advisors, and the arrival of the International Brigades. The analysis follows developments in the government of Francisco Largo Caballero, a socialist who opposed the Communists and eventually broke with Moscow.

Part 5 looks at the struggle in Barcelona and Catalonia, analyzes the fall of Largo Caballero, and traces the emergence of the government of Juan Negrín, a sympathizer who, according to Bolloten, allowed the Communists to gain control of republican Spain.

The epilogue deals with the demise of the revolution under Negrín from May 1937 to the end of the war. Bolloten breaks this period into two parts: one in which defense minister Indalecio Prieto attempted to stem the military power of the Communist party until his ouster in April 1938; the other comprising the final months of the war, military defeats, the overthrow of Negrín and his Communist supporters by left moderate forces who unsuccessfully attempted to negotiate a settlement without reprisals.

These events are portrayed through a mosaic of quotations from speeches and writings of participants, statements from ephemeral periodicals representing various political factions from right to left, and important secondary analyses. Primary sources—memoirs, letters, interviews, manifestoes, and other documents—thus draw the reader close to the drama of revolution and civil war which Bolloten himself observed 40 years ago as a press correspondent.

This book is augmented by five maps which show the political divisions at various moments of the Civil War. In addition there are three appendices, a listing and description of the major participants, exhaustive notes, extensive bibliography, and a comprehensive index of some 50 pages.

In his foreword Raymond Carr states that this book is "a mine that will be worked over by subsequent historians. They may not accept all his judgments, but they will remain in his debt." Bolloten's work clearly is a major contribution to an understanding of the Spanish Revolution and Civil War. It is essential reading for all students and scholars interested in European and Spanish developments during the thirties.

RONALD H. CHILCOTE

University of California, Riverside

Socialist Labor and Politics in Weimar Germany: The General Federation of German Trade Unions. By Gerard Braunthal. (Hamden, Conn.: Archon Books, 1978. Pp. 253. $20.00.)

Couched inconspicuously in the back pages of this impressive study of the socialist unions in Weimar Germany, primarily the ADGB, is their epigraph: "They promoted the gradual development of a democratic and social ethos in a nation not steeped in it historically" (p. 179). Gerard Braunthal analyzes this development from two broad perspectives, the historical and the topical, both of which are concisely drawn together in a concluding theoretical chapter.

The historical section, formidably buttressed by archival data and postwar as well as recent interviews with veteran union and socialist leaders, is segmented into "the formative years" of the Imperial era from the second half of the nineteenth century until 1918; the first tumultuous period of Weimar from 1918 to 1923; and the second period of political malcontent from 1929 to 1933. The relatively stable years under Stresemann diplomacy from 1924 to 1929, when the unions fared moderately well, are restricted to the analytical periphery because of the absence of critical patterns of change.

Beginning with the early repression of the unions and their rapid rise to "a respectable 'Establishment' position during the war" (p. 31), Braunthal traces the divergent phases of growth, the factional splintering, the decline and, during 1933, the final demise of the "free" (i.e., socialist) unions. He particularly emphasizes the shifts in balances of power and closeness of cooperation in the relationship between the ADGB and the SPD. The ongoing sense of fraternal cooperation between the unions and the SPD in their pursuit of pragmatic reforms within the capitalist system and of the long-range goal of economic democracy is outlined and juxtaposed to intermittent periods of union-SPD friction, in particular the unemployment issue in 1930 and, in 1932, the enamored stance of the ADGB toward Chancellor Schleicher.

A second major focus of the historical

analysis are the dynamics of internal policy choices within the free unions. Although there was a general configuration of uniform strategies and policy initiatives during Weimar, ranging from the forceful lobbying of government officials to strikes and mass demonstrations, the unions also suffered periods of turmoil and indecisiveness about goals and tactics. Braunthal poignantly questions the irresoluteness and ready political retreat of the union elite in times of stress during the 1932 Papen coup in Prussia and the Nazi consolidation of power in 1933.

The topical section of the book explores (1) the overlapping dimensions of influence between the unions and more conventional political units, i.e., the SPD, the Reichstag and the executive, (2) the organizational structure and weighting of power within the ADGB; and (3) the evolution of union economic policy. This last area includes cogent summary evaluations of the stance of the unions toward the eight-hour day, unemployment insurance, reparations, socialization and works councils, as well as long-range union interpretations of economic democracy and socialism.

Perhaps the most incisive section of the book is the brief concluding chapter, in which Braunthal assesses the plural dimensions of socialist labor in Weimar Germany and bonds together his assessments within a broad theoretical framework. Here he presents the thesis that although the success or failure of the socialist labor unions was determined largely by (1) whether the political and economic climate was favorable and by (2) whether socialists or nonsocialists were the leading policy makers, these factors did not affect union tactics and strategy. (A question emerges, however, regarding the tactical indecisiveness of union leaders during 1932 and 1933, as well as their problematic refusal to assume the reins of government, including the chancellorship, immediately after the 1920 Kapp Putsch. Do these incidents belie the thesis or constitute minor exceptions?)

Given the original division of the book into two separate sections, there remain some difficulties of theoretical exposition, which are only partially allayed by the definitive insights of the final chapter. Causal links to union goals, policies and strategies are not always clarified and might be more easily comprehended within a framework that integrates rather than dichotomizes the historical and topical investigations.

By contrast, Braunthal adopts an inclusive approach for other facets of his analysis. He draws useful cross-national comparisons among labor movements, i.e., the British and Scandinavian models versus union organization on the continent, and also presents where relevant, as on the issue of codetermination, linkages between Weimar and contemporary West German union politics.

In broad measure, Braunthal's study represents not only a needed and compelling addition to the literature on German trade unionisn but also, and perhaps more importantly, a generous contribution to our understanding of the process of fostering the "democratic and social ethos" that was noted at the outset of this review. In that sense, the book indeed may be easily extended beyond the time range and geographical limits of Weimar Germany.

ZELIME AMEN WARD

University of Texas, Austin

The Economic Constitution of Federal States. By Albert Breton and Anthony Scott. (Toronto: University of Toronto Press, 1978. Pp. viii + 166. $10.00.)

The Economic Constitution of Federal States is a significant contribution to the contemporary genre of literature that has in recent years appropriated to itself the mantle, if not always the title, of Political Economy. This literature is basically characterized by the application of neoclassical economic theory *qua* technique to problems that are, at least nominally, political. As such, the volume shares in both the virtues and defects of this school of thought.

Its virtues, in my judgment, arise mainly from the rigor lent to the analysis through the apparatus of mathematized economic theory. Its generic defects arise mainly from the distortion, and too often the trivialization, of the political dimensions of a problem that are necessary in order to bring this apparatus to bear. Although Albert Breton and Anthony Scott emphasize in the introduction that theirs is to be an essentially economic approach, this doesn't amount to much of a concession, since they soon go on to suggest that the only issues then remaining are legalistic or semantic. Indeed, it must be accounted somewhat remarkable in a book that describes itself as a basic analytical inquiry into the logic of federal political structure that one searches in vain for so much as a reference to the relevant classical literature in political economy (e.g., Hume, Smith, Hamilton, Madison, Tocqueville) or to the relevant contemporary literature that strives to incorporate modern economic theory into a nontrivial political analysis (e.g., V. Ostrom).

Criticism at this level aside, the volume is certainly one of the better analyses of this sort in the literature.

The early substantive section of the book is devoted to the explication and operationalization of a "centralization coefficient" that is designed to reduce certain structural characteristics of a public sector to a scalar measure. Although it is highly debatable whether this device plausibly measures the extent to which a public sector is "federal" (failing to capture, as it does, the nuances of overlapping jurisdictions and competitive authority and the consequent effective constraints on official power that are the heart of federal political systems), it is nonetheless an interesting and possibly useful measure of administrative centralization. There are some loose ends here. For example, the range of the index is never formally established (though, since it is a non-negative ratio of sums of products to products of sums, the Cauchy-Schwartz inequality suffices to constrain it between zero and one). The particular form of the index is not justified either analytically or intuitively, but by appeal to example (examples, I might note, containing computational errors). Most disappointingly, the index, once developed, is never explicitly integrated with the subsequent analysis. That is, although the symbol for the index is the key parameter in the equilibrium analysis that is the core of the book, its operational form never appears in that analysis. One is left with the feeling that the development of the index is essentially gratuitous, and should have appeared, if at all, as an appendix and not as a lead chapter.

The theoretical core of the book consists in a simple equilibrium analysis in which the magnitude and distribution of various sorts of costs (signaling and mobility costs for ordinary citizens, administration and coordination costs for government) are specified as a function of the degree of public-sector centralization. The model is then subjected to comparative static analysis as its specification is systematically made more realistic through taking into account the incentives and capabilities of the various types of actors in the system. The analysis here is often imaginative and interesting and occasionally permits some nontrivial conclusions (such as the stability conditions for the system not to degenerate towards completely monocentric or Balkanized extremes).

The third section of the book consists of a series of topical analyses conducted more or less in light of the previously specified model. The quality here is uneven and the relevant point not always clear, but the analysis of the role of interjurisdictional grants is well done and suggestive of possibilities for useful research.

Overall, the book is rather disjointed, and is often awkward in development. As a single volume, it is not well motivated. Some strong editing would have helped. Substantively, it must be seen as a noteworthy contribution to the contemporary literature in the area.

VERNON GREENE

University of Arizona

The Urban Question: A Marxist Approach. By Manuel Castells. Translated by Alan Sheridan. (Cambridge, Mass.: MIT Press, 1977. Pp. x + 502. $20.00.)

For some time now, efforts have been under way in the major advanced countries to reconcile the urban movements of the 1960s and 70s, together with the larger "urban crisis," with the Marxist intellectual paradigm. This development has produced many creative and provocative variants. The net result has been a reinvigoration of the literature on urban politics.

Because France has had both a strong history of left-wing party activity and a strong Marxist intellectual tradition, it has produced one of the richest examples of this "new urban politics." Although the scholars who have built up this literature in France are, as one might expect, a varied and highly polemically skilled group, Manuel Castells is perhaps its best-known and most prolific exponent. Castells is an exiled Spanish intellectual who has managed to climb to the pinnacle of the French academic system—the Ecole des Hautes Etudes. Given French ethnocentrism, this in itself was a major accomplishment. Well shy of 40, Castells has published, at last count, eight major books on subjects ranging from urban renewal and urban protest to regional economic development and Latin American urban politics. He has taught for varying periods in Canada, the U.S., and Chile, and maintains an extensive network of contacts in Spain and Italy as well as throughout Latin America. In *The Urban Question,* Castells presents the initial theoretical framework in which he intended, as of 1972, to situate his subsequent research efforts. The book is important because, however flawed, it remains just about the only such general theoretical effort the French Marxist urbanists have produced. (Castells has appended an afterword to the 1977 English edition which criticizes his own formulations. For those wanting to extract the nub of Castells' argument from this difficult-to-read book, this is the place to look.)

The urban crisis has presented Marxism with an interesting challenge because its major themes—spontaneous political action, a base in ethnic neighborhoods rather than the workplace, and an emphasis on the stability of urban service bureaucracies—have only a minor, "secondary" place in the traditional Marxist intellectual constellation. One looks to this book, then, for answers to one basic question: how can we get from the structure of the capitalist economy (in which a class structure based on roles in production has theoretical primacy) to the specifics of urban political actions (often based on multi-class, residential social bases) and back again? The book stands or falls on how well one thinks it does this job both on theoretical and illustrative or empirical grounds.

Castells does three things to make his case. He first criticizes urban theories, primarily the Tonnies-Simmel-Park-American urban sociology school, which do not center their analysis on the capitalist mode of production. This purely critical part of the book is the most compelling, although it operates on a more purely epistemological plane than suits my taste. Secondly, Castells retools the French structuralist Marxist conceptualizations advanced by Althusser, Balibar, and Poulantzas, for use in the urban setting. This part of the book is least convincing, because it appears mechanistic, static and purely classificatory. Reliance on the structuralist intellectual inventory has led Castells to decorate his book with terms which superficially appear to be scientific but actually hide a great deal of vagueness. Words like "correspondence," "instance," and "conjunctural effect" are among the worst of the offenders, and the whole terminology will tend to thwart, and perhaps infuriate American readers. Indeed, the Althusserian penchant for trinitizing each mode of production into economic, politico-juridical, and ideological "instances," and further for decomposing any given stage of development into the "conjunctural" interplay of its different coexisting modes and instances will strike many as resembling Parsons' A-G-I-L scheme. (Whether the resulting mechanistic formalism can be entirely or only partially attributed to Althusser himself, I am uncertain; in his afterword, Castells himself criticizes his "extremely formalized theoretical system" as "merely a classification grid.")

Finally, the book turns to a third task: applying these conceptual distinctions to a series of case studies in urban planning (urban renewal in Paris and the United States) and urban protest politics (in Paris, Montreal, Chile, and the U.S.). Because he introduces urban politics as a subject whose origins may be understood in structural terms and whose impact may produce "a qualitatively new effect on the social structure" (p. 262), one hopes for far more from Castells than one gets. On the determination side, Castells merely asserts that urban planning initiatives arise from problems which "run counter to the interests of the dominant classes and disturb the functioning of the urban system," and which therefore "demand intervention" (p. 287). He goes on to explain planning interventions by their effects, a methodological faux pas which is only the most noteworthy of many. Castells' path model diagrams will cause American readers, grounded as they are in empirical analysis, to shake their heads in disbelief. On the political response side, Castells' cases of protest only weakly show structural determination. In short, Castells' empirical illustrations of his theoretical apparatus only further undermine the reader's faith in Marxist structuralism of the Althusserian brand as a useful tool for explaining urban planning and politics.

But if *The Urban Question* fails in its basic quest to take us from economic structure to urban political action and back again, it still makes three important contributions. First, its critique of environmental determinism in urban studies is devastating. Castells lays to rest the notion that "the city" or "the urban" can itself be an actor or an explanatory variable. Environments only provide a structured context in which actors engage in practices and make choices. Secondly, Castells opens the way for a real advance in Marxist theory by arguing that politics, though secondary in theory, is primary in many practical circumstances and must be placed at the center of scientific explanation. Though Castells' structuralist formalism does not provide a convincing vehicle for this type of explanation, he at least points out the central theoretical problem. Finally, almost against his will, the strength of Castells' empirical material carries him from the realm of production-generated classes to the realm of collective consumption interest groups. It is in the attempt to relate the politics of reproduction systematically and theoretically to the politics of production that Castells promises to make his greatest contribution both to Marxist theory and to urban studies. By claiming that reproduction and collective consumption lie at the heart of the urban question, Castells has taken a bold step which orthodox critics will probably say has taken him too far towards Max Weber. For ne, though, it is a journey to be encouraged and watched closely.

JOHN MOLLENKOPF

Stanford University

The Soviet Theory of Development: India and the Third World in Marxist-Leninist Scholarship. By Stephen Clarkson. (Toronto: University of Toronto Press, 1978. Pp. xii + 322. $17.50.)

The August 1979 convocation of the International Political Science Association in Moscow underscored the increased presence of Soviet social scientists in international scholarly nterchanges. As the novelty of participation by the Soviets finally begins to diminish, we can hope for a more sober assessment of the contribution of Soviet social science as it has been applied to the analysis and understanding of important global issues. Stephen Clarkson's book is a valuable step in this direction, since it attempts to examine Soviet scholarly writings "at their face value and . . . in their own terms as intellectual contributions" to the understanding and possible solution of problems of Third-World development (p. 6).

Since the quantity of Soviet material on the Third World has burgeoned in the last two decades, Clarkson has chosen to focus his evaluation on Soviet "political economy" writings on India, the country which has occupied the central place in Soviet development theory. However, he seeks to distinguish his own analytical focus from that of other recent monographs on Soviet policy toward India. Rather than examining Marxist-Leninist doctrine at the level of strategy for the communist movement or for Soviet foreign policy, Clarkson instead aims to concentrate on its "developmental and theoretical significance" (p. 15).

Clarkson is to be commended for his scrupulous fairness to the Soviet authors whose works he reviews. Indeed, his orderly organizational framework and lucid exposition impart to the "Soviet theory of development" a greater sense of coherence and stability than the original works themselves possess. In the process, the reader loses sight of the dynamic quality of a frequently changing Soviet analysis. Moreover, even though Clarkson does differentiate between the approaches of "radical" and "moderate" scholars, he makes little effort to convey the distinctive biases or relative scholarly or political standing of individual Soviet scholars. Another shortcoming in Clarkson's approach is the neglect of Soviet studies of Third-World countries (such as those in tropical Africa) that are less capitalistically developed than India; the most notable examples of "creative Marxism-Leninism" are to be found in development studies not covered here.

These deficiencies in scope and method are far outweighed by the sound scholarship and penetrating analysis contained in this monograph, which is the product of 16 years' research. Despite his efforts to evaluate it as serious scholarship, Clarkson has found in Soviet development theory a web of contradiction and confused ideological thinking proceeding from a "crude they-are-bad-we-are-good" approach that "leads to a whole series of positions unacceptable to the non-believer" (p. 193). Soviet writers employ Marxist-Leninist concepts that are both value-laden and poorly suited to Asian or African social structures; the products of their scholarship are thus characterized by "a vocabulary and style of writing that makes their output as distinctive as it is dismissable" (p. 251). Policy conclusions are shown to follow not from independent scholarly analysis but from prior political judgments of the "progressiveness" of a given regime's policies toward the USSR.

If, as Clarkson concludes, the vast body of Soviet writing on the Third World has only limited significance as a model or theoretical guide for development, does it have greater impact in other realms? In his estimate, such scholarly output is not a reliable guide to Soviet foreign policy, has no real impact on Soviet foreign policy making, and has little influence on foreign communist parties. Clarkson concludes on the basis of interviews with leading Indian opinion and policy elites that "neither in theory nor in practice have the Soviets had any noticeable impact on Indian elites' ways of thinking or acting" (p. 262).

Clarkson's research thus supports the conclusion that the wells of "creative Marxism-Leninism," as directed toward analytical and policy issues of Third-World development, have long ago run dry. This conclusion, while hardly novel, is here given added credibility both by the solid research on which it is based and by the genuine effort of this Canadian political scientist to give Soviet scholarship the benefit of the doubt.

Sadly, the findings of this study give no reason to think that the vision of genuine mutual interchange and learning between Soviet and Western social scientists will be quickly or easily realized. Clarkson's own conclusion is a sobering warning to Soviet scholars of development who seek to have their works taken seriously outside their own country: "Until quality prevails over quantity in the research institutes of the USSR, they will be more likely to serve as an uncertain guidebook for watchers of the Kremlin than as an alternative paradigm for students of the third world" (p. 268).

ROBERT H. DONALDSON

Vanderbilt University

The Difficult Flowering of Surinam: Ethnicity and Politics in a Plural Society. By Edward Dew. (The Hague: Martinus Nijhoff, 1978. Pp. 234. Guilders 37.50, paper.)

This book throws light on how Surinam developed into a viable democracy through an in-depth analysis of the political system of this small tropical country of many nationalities and languages. It is an unfortunate characteristic of American students of comparative politics to avoid research subjects—when they do not touch upon an aspect of immediate significance in present-day world affairs—in which the primary resources are in unfamiliar languages. It is, therefore, a pleasure to see that this barrier has not impeded Edward Dew in his thorough analysis of the intricate political relationships of multiracial Surinam. Archival research and perusal of the press of this nation demanded the knowledge of languages not generally taught in North American universities.

The ethnic mosaic of this former colony of the Netherlands, on the Atlantic coast of northern South America, has caused it to have a bewildering political composition of parties and factions which on first glance give an impression of dangerous instability. Serious racial, religious, and ideological differences made Surinam's quest for independence, in the early seventies, appear to be a pursuit of doom. Many political observers had grave doubts about its future. There was a belief that the three major racial groups, the Creoles, the Hindustanis, and the Javanese, would be locked in fraticidal conflict. Independence would not bring genuine freedom, but riots and disorders more calamitous than those of neighboring Guyana. The final consequence would be an oppressive authoritarian regime. Happily for Surinam, these predictions of catastrophe proved false. Sovereignty did not bring dictatorship, but on the contrary, a modern and vigorous democracy.

Why did democracy, which has failed in so many young Third-World countries, flourish in Surinam? This is the question the author poses himself. Before answering it, he reviews Surinam's political history. He examines first how Dutch colonial viewpoints on economy affected geographically and socially the settlement of the country; second, how shifting colonial policies with regard to ethnic issues caused differences within racial blocs which in later years forestalled easy transformation of ethnic strength into political power; third, how parties based on modern ideologies had little chance of success among the Asian groups, the Hindustanis and Javanese. Instead of attempting to erase cultural differences, colonial rule had accentuated them. Preservation of ethnic and religious identity, therefore, played a more important role in the politics of these groups than a desire to reshape society and better one's individual economic position according to the appeals of new political theories. At times, this attitude gave Surinamese politics a rather rigid conservatism which, it would seem, could make interracial cooperation impossible and could become the cause of a violent explosion. That this did not happen the author attributes to the party system. Economic and cultural factors, and in part the electoral system, caused ethnic groups to be represented by myriad parties. To govern, both the large Creole and the large Hindustani party needed the support of several smaller ones. Governments were, therefore, in nearly all cases coalitions in which all racial groups were represented. This fact compelled these governments to follow policies accentuating interracial cooperation. Dew explains how this is done very well, and his analysis of party elites is admirable.

I would have been interested in a more profound study of the factors that give Surinam's present parliamentary system such strong legitimacy in the eyes of the people. It is important to realize that the competing political elites are quite sensitive to accusations of not having observed the rules of the system. Unlike the people of many other developing countries, the citizens of Surinam have a deep respect for their democratic institutions. Tampering with the checks and balances provided by the Constitution is not to be permitted either by a charismatic leader or a "man on horseback." I hope that this writer whose enterprising and meticulous pioneering scholarship has highlighted so many facets of ethnic politics may decide to examine further the sociocultural roots of the legitimacy of Surinam's constitutional system.

ALBERT GASTMANN

Trinity College

Distrust and Democracy: Political Distrust in Britain and America. By Vivien Hart. (New York: Cambridge University Press, 1978. Pp. xvii + 251. $19.95.)

This is a puzzling book. Vivien Hart addresses one of the most important phenomena of contemporary mass politics: the rise of significant, pervasive, and highly vocal discontent with the political process, with political leadership, and with policy outcomes in Great Britain and the United States. The puzzle begins with organization. At the outset Hart develops a sharp distinction between political distrust on one hand—the unfavorable but quite rational evaluation of politics by citizens judging reality in terms of the "mainstream" norms of a democratic political culture—and alienation, apparently identical in her view with anomie, on the other. The analysis thence proceeds to a detailed discussion of certain contemporary survey data, coupled with a blistering attack on Jack Dennis' gloomy conclusions about the rejection of existing politics by British youngsters. Then we abruptly turn to the book's core, two chapters on nineteenth-century "radicals," Kansas Populists circa 1890 and the "rank-and-filers" of the Birmingham Liberal caucus in the mid-1880s.

Hart's purpose seems clear. Her central proposition is that people who are distrustful of politics are essentially rational actors reacting to a perceived gap between democratic promise and actual political performance. But that democratic promise is not somehow alien to the established political culture, still less a radical critique of its assumptions about power, authority, and human nature. Rather, it is of the essence of the mainstream. This proposition leads to two important conclusions. First, the increasingly influential school of "democratic elitists" have missed the point about protest from the distrustful. This protest is in no sense a genuinely radical attack on the existing political order. Accordingly, it is not to be misunderstood as a serious threat to existing institutions, nor should political elites and the intellectuals who support them use this protest to express their fears about the "crisis of democracy," but rather as an occasion for self-criticism: rational protest by the distrustful (who are not the prisoners of various worrisome psychological disorders) arises when the elite themselves, by their actions, widen the gap between promise and delivery. Second, survey analysts and other political scientists should be very careful to avoid the apocalyptic, or to inflate the significance of expressions of political disaffection.

This argument contains much truth. No doubt the "rank-and-filers" of the Birmingham Liberal caucus did not view politics as the primary vehicle for social change. Similarly, much of Populist "radicalism" was hardly alien to very important aspects of nineteenth-century American political culture. Also, this study duly—and accurately—confirms well-known differences in American and British political cultures. Students of politics, no less than worried elites unused to being challenged by sectors of the public, may well remember that organized political protest in these countries has always reflected some features of an ambiguous but hegemonic political code. The larger this movement, the more faithful will be this (albeit skewed) reproduction of traditional mainstream themes.

Yet how far does such analysis take us? This study is vulnerable to a number of criticisms. For example, while the American Populists were no doubt authentic representatives of an American political tradition, they also were engaged in the most serious critique of emergent corporate capitalism and its march to hegemony which the United States has ever produced, as witness the behavior of frightened contemporaries in the industrial heartland. See the recent and magisterial analysis of this movement by Lawrence Goodwyn's *Democratic Promise* and Louis Galambos' *Image of Big Business in America, 1880–1940.* To say that Populists were, after all, all good American liberals would miss the point even if that were wholly true.

Another problem is the extraordinarily isolative character of this analysis, if genuine concern for historical dynamics is involved. First, Hart largely concentrates on criticisms of politics and the "growing complexity of government" around 1890, and in doing so misleads us as to the basic point of the critique. This was not governmental complexity but the utter domination of government by corporate capitalism and its agents. While some of the rhetoric dealt with legal-institutional boundary problems (such was currently the dominant mode of public discourse), the Populists which Goodwyn describes did not see political institutions in splendid isolation from the socioeconomic uses to which they were being put, or *could* be put.

Connected to this is the failure of more than a tendentious link between past and present. For instance, the 1890s Populists were a small minority, never winning more than 11.5 percent of the total national vote. If we assume that the distrustful expressed themselves in protest movements in electoral politics, then at least five-sixths of the very fully mobilized electorate of that era would have to be excluded. If we assume a much larger proportion of the distrustful, then we need to explain the extraordinary turnouts and the equally extraordinary devotion to their major parties displayed by the overwhelming majority until the general smashup of 1894–1896. In any case, as the Harris data shows (p. 69), in the 1970s more than a majority of the entire adult population believe themselves "alienated and powerless." Clearly, the "distrustful" are now to say the least very numerous—almost certainly a larger proportion of the electorate than in the 1890s. One of the largest turnout rates of all time for an off-year election occurred in 1894, when 73.4 percent of the electorate outside the South voted. Not surprisingly, the smallest of all time was reached in 1978, when the non-Southern turnout rate hit 37.9 percent of the potential electorate.

As I have often said, American politics in the late nineteenth and late twentieth centuries are very different breeds of cat. Then, disenchantment with the existing political order led to intense mobilization through parties. Now, it leads to the dissolution of parties and massive, heavily class-skewed abstention from voting. Parties, it is commonly supposed, are important integrative and communicative links between rulers and ruled. Their dissolution in the current era raises problems of a kind never seen in the past 150 years, and approximated only during the height of industrial-capitalist hegemony in the 1920s. In this context, Hart's reflection makes strange reading: "The outburst in America in the 1960s was a return to normal rather than a dramatic collapse of normative integration—the distrustful there are not philosophical extremists but are true to the mainstream of the American political tradition" (p. 182).

This statement gets us near the heart of the problem. Like many others, this statement has its grain of truth. But in a broader context it is profoundly misleading—as it is misleading to imply that, because Populists were in the American democratic tradition, they and their critique of the emergent corporate political economy were non-threatening to the established elites in that political economy. This study's flaws are ultimately conceptual flaws. It lacks a sense of historical transformation, of the links between government and dominant economic power which occasionally give rise to protest among the distrustful, or of the fundamental implications for the problem of distrust caused by the very recent change in what the state does *to* as well as *for* the citizenry. One is left with the impression that "distrust" operates in a narrowly political and wholly timeless continuum, and that it is wholly non-threatening.

This is all rather precisely liberal, to be sure. But mainstreams are both very broad and ambiguous affairs. Who can doubt, after all, that Hitler and the Ayatollah Khomeini reflected important elements in the mainstreams of their respective cultures? The analysis of such right-wing revitalization movements would require quite different analytic tools than are provided by the liberal academic mainstream represented here. So, one suspects, would the analysis of the etiology, scope, and implications of the *present* mass hostility to "politics" in the United States and Britain.

WALTER DEAN BURNHAM

Massachusetts Institute of Technology

Housing Policy in the Developed Economy: The United Kingdom, Sweden and the United States. By Bruce Headey. (New York: St. Martin's Press, 1978. Pp. 276. $21.95.)

In this valuable survey of the development of housing policy in Sweden, Britain, and the United States, Bruce Headey shows how divergent policy outputs among "similar systems" can be. To summarize his conclusions briefly, Headey finds that in Sweden the government has achieved high levels of housing construction and design along with a major redistribution of housing services and costs in favor of working-class families. "The position has been reached in which the dwellings and neighborhoods of most working-class families are of comparable quality to those of most middle-class families. Furthermore, due to state subsidies and housing allowances, working-class people pay less for their housing than middle-class people" (p. 229). There has been a coincident effort (at least until recently) to encourage the construction of multi-family rather than single-family housing.

The contrast with the other countries reviewed is sharp. In Britain, the more modest aim of security Beveridge-type "minimum standard" housing may have been achieved. The price in financial resources and political controversy, however, has been high. Working-class housing remains qualitatively distinct. And subsidies as a whole continue to benefit middle-class homeowners over working-class renters. In the United States, tax subsidies and timely intervention during the Great Depression have so far helped to promote a life of suburban ease for the majority of middle-class Americans. The cost has been borne by the less well-off and by those currently without a private home. To use Headey's terms, vertical and horizontal inequalities have been increased.

The great merit of this book is to present and contrast these developments clearly and persuasively. The argument is further advanced by clever calculations of rates of return on housing and by some trenchant criticism of British housing policy in particular. (I have the impression that Headey considers American policy hopelessly regressive. He could well be correct.) Finally, Headey outlines proposals that would move housing policy away from providing disproportionate assistance to higher-income families, which, to repeat, is presently the rather perverse situation in Britain and the United States. These proposals are carefully thought-out and are derived logically from the main body of the text.

What the book accomplishes less successfully is the elaboration of an explanatory scheme to account for the differences that are described so well. Plausible explanations abound—Headey mentions divergent orientations of the major "left" party in each country, institutional constraints, the presence or absence of one-party dominance, ideology (public opinion), differing government-interest group interactions. These ideas, however, are not linked or ordered systematically. To approach the matter from another direction, since Headey does not relate housing policy to other policies or to patterns of state action, the significance of his findings is less clear than might be hoped. This problem, I must note, is not relieved by the rather ad hoc use of James Q. Wilson's categorization of policy programs in cost-benefit terms. These reformulations, when not merely intrusive, tend to obscure more than to enlighten. The elements of a compelling explanation need more rigorous articulation.

In sum, while Headey may fail to advance the at best nascent attempts to develop appropriate theories, in outlining what has happened and how, he is superb. The book warrants wide attention as one of only a few comparative policy studies with a purpose. This attention would be facilitated by issuing a paper edition which would help generate the audience, particularly among students, that the book deserves.

NORMAN FURNISS

Indiana University

Elections Without Choice. Edited by Guy Hermet, Richard Rose, and Alain Rouquié. (New York: Wiley, 1978. Pp. xi + 250. $14.95.)

Most of the publications of the Committee on Political Sociology of the International Political Science Association and the International Sociological Association that have appeared so far have concentrated on the political and electoral processes of Western countries. The present volume deviates from this pattern by focusing on the function of elections, prevalent in the non-Western world, that are not free and competitive. It is the first full-length and broadly comparative book on "elections without choice" following the earlier shorter analysis by Richard Rose, one of the editors of the book under review, and H. H. Mossawir in the journal *Political Studies* in 1967 (Vol. 15) and the pioneering articles on sub-Saharan African elections by a number of scholars, some of whom also contribute to the present volume: Joel D. Barkan, John J. Okumu, and Denis Martin.

Elections Without Choice begins with two analytical chapters. Guy Hermet presents a conceptual framework for the study of state-controlled elections, and Alain Rouquié discusses the nature, types, and functions of elections that are not controlled by the state but that are nevertheless not really competitive as a result of the strength of clientelist relations. Of the other eight chapters, five are case studies and three are comparative treatments of groups of similar countries. The single-country analyses deal with Cameroun (authored by Jean-François Bayart), Kenya (Joel D. Barkan and John J. Okumu), Tanzania (Denis Martin), Syria (Elizabeth Picard), and Portugal (Philippe C. Schmitter). Juan J. Linz's discussion of non-competitive elections in Europe first deals with the crucial semi-free elections in Germany in March 1933 and in Italy in April 1924 that helped to establish dictatorial rule in these countries, but then becomes a wide-ranging analysis, not strictly limited to Europe, of elections in totalitarian, authoritarian, and pseudo-constitutional regimes. It partly overlaps Alex Pravda's chapter on Communist party states which is mainly based on evidence drawn from five countries: the Soviet Union, Czechoslovakia, Poland, Hungary, and Yugoslavia. Richard Rose's final chapter is a paired comparison of elections *with* choice but without full legitimacy and consent in Northern Ireland and the American Deep South.

It is very useful to be reminded of the fact that competitive free elections are the exception rather than the rule in the contemporary world. Almost all states conduct elections of one kind or another but, as the editors emphasize, elections without choice represent "the ballots most frequently held" (p. vii). Secondly, although it is tempting to dismiss such elections because they do not perform the crucial functions of free elections in Western democracies, "the fact that elections do not have the same meaning when they are without choice is not evidence that they lack any meaning. Instead, it is an indication that their meaning is different" (p. vii). For instance, Hermet identifies four functions of state-controlled non-competitive ballots: communication, education, legitimation, and the adjustment of the internal equilibrium of the governing circles (pp. 13–17). And it is also worth remembering that elections do not have to be perfectly free and competitive in order to give the voters at least a *degree* of democratic control.

The three criteria that Hermet uses to distinguish between democratic and non-democratic elections are unexceptionable: freedom of voters, competition between candidates, and a meaningful effect on government policies. However, the way in which the authors interpret and apply these criteria is often unrealistically restrictive and betrays a strong inclination to adopt the Westminster model of democracy as the ideal norm—a surprising bias since almost half of the volume's contributors are French. For instance, government policies can be profoundly affected by the outcomes of elections even when these do not entail "replacing office-holders by leaders of the opposition," which Hermet calls "the fundamental rule of elective representative democracy" (p. 4). A similar narrow interpretation is offered by the three editors in the preface when they imply a condemnation of elections in multiparty systems because these may not offer "a verdict upon a government" but only the choice of representatives to negotiate a coalition government (p. viii). And they list "an executive responsible to an elective body" as one of the preconditions for democratic elections—thus unnecessarily excluding legislative elections in American or French-style presidential systems (p. viii). It is equally unnecessary to interpret the criterion of voters' freedom as requiring the absence of "categories dividing the electorate and revoking the idea of popular sovereignty" (p. 3). Geographical divisions of voters are a common feature of democratic elections, and it is not clear why non-geographic divisions (e.g., ethnic categories) should be considered inadmissible if each category is entitled to a fair share of the total representation. In fact, an explicit recognition of the need for special rules for minority representation may be the only way to remedy the systematic suppression of minorities to which majority rule may lead in severely divided societies such as Northern Ireland (see Rose's discussion, pp. 206–10). A final example is Rouquié's unqualified labeling of election systems in divided societies with a high degree of "vertical solidarity," in which voters naturally divide themselves into ethnic parties, as "structurally authoritarian" (p. 22).

These conceptual and definitional problems indicate that *Elections Without Choice* is not the definitive comparative work on the subject. But its empirical breadth as well as its theoretical depth and originality make it a highly significant initial global-level exploration of the diverse functions of elections in different types of regimes.

AREND LIJPHART

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Civil-Military Relations in Communist Systems. Edited by Dale R. Herspring and Ivan Volgyes. (Boulder, Colo.: Westview Press, 1978. Pp. xiii + 273. $21.00.)

This is a collectively authored volume whose title is self-descriptive: it is about civil-military relations in communist systems. Like most collectively edited volumes, the quality of the chapters varies considerably. Like all too many collectively authored volumes, several of the articles have appeared elsewhere. Unlike many edited volumes, fortunately, it has a theme; Dale Herspring and Ivan Volgyes have put together a book, not a non-book. Its theme, namely, the nature of party-military interactions in communist systems, is nicely told by three of the better statements about those interactions in the Soviet context. Roman Kolkowicz's image of Soviet civil-military relations as entailing constant conflict between "two definable and dichotomous institutions" (Dale Herspring's introduction, p. 1) is presented. William Odom's view that the military and the CPSU are similar and highly interpenetrated institutions is also reproduced. Finally, Timothy Colton articulates a participatory model which envisages the military as an institution whose interests have thus far largely been satisfied by the Communist party. In particular, Colton asserts the party has given the military a sense of participation in decisions, has allowed for the considerable self-regulation of the military, and has generally pursued policies that are congruent with military interests. These alternative perspectives are followed by country studies treating the Soviet Union, the German Democratic Republic, Hungary, Rumania, Yugoslavia, Cuba and China. Jaded peruser of collective volumes that I am, I am pleased to report that several of the country studies actually make reference to the general perspectives articulated by Kolkowicz, Odom, and Colton. In short, the editors have rendered a useful service by bringing together a reasonable sampling of articles dealing with civil-military relations in communist systems, a collection that should prove useful for advanced courses in comparative communist systems.

What the book does not accomplish, however, should also be noted, if only to identify what ought to be a major item in the research agenda for Soviet and more generally communist studies. The volume presents three of the major plausible alternative perspectives on the communist military: as an interest group ranged consistently against the party qua apparat, as an institution indistinguishable from a thoroughly militarized society, and as a participating institution whose support is cultivated by the party leadership. The volume fails, though, to provide much by way of empirical evidence with which to assess the relative explanatory potency of the three perspectives. There are numerous methods by which this task could be accomplished. An impressive example of one way the behavioral consequences of the alternative perspectives could be specified and then evaluated against the realities of communist civil-military relations is provided by William Ting's article on Chinese factionalism in the June 1979 issue of this *Review* (73, pp. 478–93). Whatever the chosen methodology, however, it is only through such efforts that scholars will be able to develop criteria for preferring one perspective over others, or, even better, for specifying the circumstances in which, for instance, party and military interests are likely to conflict, when some party apparat elements and some military elements are likely to clash with other groupings within the party and the military, when the military and the party are likely to coalesce against other key actors in communist political systems, and when (if ever) it makes much sense to think of communist systems as permeated by the military.

WILLIAM ZIMMERMAN

University of Michigan

The Political Economy of Inflation. Edited by Fred Hirsch and John H. Goldthorpe. (Cambridge, Mass.: Harvard University Press, 1978. Pp. xi + 307. $14.00.)

We on the dark side of the Phillips Curve sense that inflation may be the existential theme of the *fin de siècle.* Technical debates about monetary and cost-push causes of rising prices have been overwhelmed by widespread public protests and tax revolts. Crises in the world political economy and energy supplies underscore the social and political nature of the inflationary experience. Prices go up, paper currencies go down and everyone searches for a financial security to counter spreading uncertainty. As our situations become inflated, we feel driven to invest ourselves elsewhere to hedge against the risk of losing our value.

Many who should know better believe that we need a seminal thinker to solve our economic crisis and inflation problems—a new Keynes. This nugget-packed volume edited by the late Fred Hirsch and John Goldthorpe suggests that this is not the case: multidisciplinary teams are better capable of taking apart our situation, even if they cannot promote a golden panacea to solve our inflation sickness once and for all. With more doctors, there is less chance that a single doctor's cure will be worse than the disease.

Inflation, argues John Flemming, is essentially a monetary ailment characterized by an overdose of liquidity provided by profligate money suppliers steered by government policy. Historian Charles Maier views inflation in twentieth-century capitalist societies in terms of distributive conflict and system stability, calling the concept of growth as a surrogate for redistribution "the great conservative idea of the last generation" and indicating that indexation appeals when "guidelines" or social compacts fail. In contrast, the communist states of Eastern Europe have largely avoided inflation over the past two decades, even though money incomes and real consumption have grown, argues economist Richard Portes. David Piachaud looks at the effect of inflation on unequal income distribution in Britain, while Alan Peacock and Martin Ricketts maintain that the size and growth rate of the public sector of a national economy are directly related to the rate of inflation. Showing stronger statistical support for his case than Peacock and Rickette, Milivoje Panić, a bank advisor, relates the rate of inflation inversely to the levels of income and consumption that a nation has achieved, but relates it positively to the degree of inequality in its internal income distribution.

In the piece by Samuel Brittan, an economic journalist, political actors—voters, bureaucrats, politicians—all "maximize" as much as they can, stimulating excessive injections of money into the economic system and threatening the existence of liberal-democratic institutions. Starting with the inconvenient "residual categories" which economists' theories of inflation often exclude, sociologist John Goldthorpe argues that high rates of inflation reflect recent changes in the form of social stratification within capitalism, which has taught the "mature" working classes to exploit their bargaining position on the market to the full. Such maximizing behavior is a justifiable expression of citizenhsip rights in a decaying status order by formerly dominated interest groups. Colin Crouch, also a sociologist, treats inflation as one of society's interest-regulation problems. The powerlessness of state institutions to confront inflation is the focus of Malcolm Anderson, a political scientist. And economist Fred Hirsch sums everything up in terms of the conflict between the classical equilibrium market ideology and Marxist analysis: "Both see the economy as governed by the imperatives of a closed system of abstract economic relationships, but where the one model programmes the variables for harmony, the other sets them for auto-destruct" (p. 273). The book is a superlative example of a cooperative sociology of knowledge stemming from the Warwick Conference of 1977 which makes much individual research appear selfish, if not inadequate, in comparison.

Inflation appears as an individualist scar in the superego of capitalist societies, resulting in part from the high expectations promised by quasi-free market systems, and representing a cover for the social costs of the gap between the demands of individual-maximizing behavior and the ability of democratic political economies to satisfy these demands. As paper money, grades, degrees and academic publications become more liquid and more liberally distributed, their individual values go down, but not the expectations of their recipients. To go against the mainstream "redistribution ideology" of inflation is non-democratic and technocratic, and such policies are unlikely to be carried through consistently enough to be effective in the politicized economies of late twentieth-century democracies.

ROBERT A. ISAAK

Johns Hopkins University

Origins of the Israeli Polity: Palestine under the Mandate. By Dan Horowitz and Moshe Lissak. (Chicago: University of Chicago Press, 1979. Pp. xii + 292. $19.00.)

In this volume two Israeli political sociologists analyze the social and political systems of the Jewish settlers in Palestine—the *Yishuv*—during the British Mandate preceding the establishment of statehood in 1948. To present a systematic, detailed and sophisticated analysis of a very complex and continuously developing society, with reference to different levels of political organization, in 250 pages of highly readable text, is by itself an accomplishment. Concerned about a proper methodological mix between a descriptive case study and a generalization-oriented social science analysis, the authors end up writing a good traditional political science study, frequent reminders of their sociological allegiances notwithstanding.

As the main organizing concept for their analysis Dan Horowitz and Moshe Lissak have chosen Shils' "center-and-periphery" model, although they admit that "the center was not the predominant source of authority . . . entrepreneurship and symbolic creativity" (p. 10). In order to interpret the coalitional structure of the "national institutions" and their coordinating role in the relationships between ideologically competing sociopolitical movements, the authors combine the center-periphery dimension with the "consociational" model of democracy. Structural models, however, do not provide explanations; indeed, it is necessary to explain why such structures developed, or why they were adopted, and this seems to be the intended theoretical relevance of this case study.

Thus, a voluntary political system lacking formal coercive authority—but entrusted with the distribution of resources received from the Jewish people in the Diaspora—inevitably had to seek an agreement between the constituent parties and settlement organizations with respect to a distributional formula for such resources. Consequently, all Zionist parties accepted—most of the time—the "authority" of the Jewish Agency Executive and the "National Council" elected by the settlers. The counterpart of this coalitional "imperative" was the need to somehow accommodate all participant political entities in order to prevent separate appeals to external factors, e.g., the British authorities or Jewish organizations in the Diaspora. Hence, the consociational structure was the result of the absence of sovereign authority. Given the intense ideological differences between the socialist labor movements, the middle-class farming and urban sectors, and the religious parties, this wall-to-wall "confederation" could not have survived without the underlying and legitimizing common nation-building goals and the fact that moderate leaders from the different sectors could cooperate on at least some of the issues. Arab violence also helped in cementing cooperation.

The book documents and analyzes the successive stages in the development of the national political center, its functions, powers and authority, as well as the role of the major "subcenters"—parties, settlement organizations and the General Federation of Labor (*Histadrut*). It then emphasizes the growth and predominance of the largest Labor party, *Mapai.* The unique role performed by the parties in a society-building as well as nation-building period is highlighted by the dual aspect of manpower mobilization: by recruiting candidates for immigration in the Diaspora and then providing for their needs after immigration to Palestine, the parties fulfilled both the Zionist task of Jewish settlement and the political task of building and enlarging their own respective constituencies. In almost every area one finds unique conditions. As the tasks became more complex, patterns of political activity were institutionalized and there was an increasing professionalization of party personnel. Yet institutions were flexible in adapting to changing conditions and within parties, relations were extremely informal. Since the pioneering ethos of a future-oriented society excluded material and status rewards, the measure of success was achievement in the nation-building enterprise. Achievement, however, was evaluated according to ascriptive, particularstic criteria: contribution to one's own party, movement, kibbutz, etc. Elite positions were interchangeable and did not carry outer signs of status, but an ideological and organizational elite developed on the basis of seniority. Much capital was invested regardless of economic considerations of profit and loss; the primary consideration was the settlement effort and the conversion of capital into political assets.

The book surveys a broad spectrum of levels and rates of development, forms of settlement, economic activity, sociocultural differences and political conflict. It also reminds the reader that no parallel processes have been observed in the nation-building experiences of other developing countries which achieved statehood in the post-World War II period. While this is true, the comparison of the *Yishuv* with these other nation-building experiences is not really legitimate. It is the post-1948 state of Israel, absorbing mass immigration from African and Asian countries, which fits the comparison much better. The pre-statehood immigrants, and especially the pioneer settlers, had been motivated by national and social ideologies which had their philosophical, social and political roots in late nineteenth- and early twentieth-century Europe. Moreover, they were attempting to build a new society, or rather, several competing models of a new society. If comparisons can be made at all, they should be sought in some of the experiences of other nations of Western immigrants (Australia, Canada, the United States). The different historical periods involved may cause such comparisons to be of limited interest, if not meaningless; but the fact that nation building in the *Yishuv* and former colonial territories in Africa or Asia were contemporaneous processes does not make those comparisons meaningful either.

It is regrettable that Horowitz and Lissak have condensed the transition to statehood into one chapter, although they accurately identify and briefly discuss the major issues. A thorough analysis of the heritage of the *Yishuv*, its differential pace of change in institutions and in political culture, combined with new functions and new decision-making rules—but with the same leadership—deserves an entire volume by itself. It would reveal the early "decay" of the center-periphery structure through a much larger concentration of important functions at the center, beginning probably no later than Ben Gurion's 1942 decision to opt and organize for full statehood and independence, and possibly even earlier. The functional division of labor between the Jewish Agency Executive, the National Council and the expanding political role of the *Histadrut*, actually contributed to the development of a hierarchy of power, especially within the parties of the Labor sector. It was this leadership, and the party structure, which survived throughout most of the independence period. And if there was a fairly rapid "routinization of charisma," it was achieved by the very same leadership, who had now turned into "conservative revolutionaries," as Horowitz and Lissak do not fail to notice.

Although the chapter dealing with "The Ideological Dimension" is outstanding, because of the depth, breadth and structure of the analysis, the entire book is not merely the only systematic introduction to the politics of the *Yishuv* available to the English reader; it is also a very good one.

MOSHE M. CZUDNOWSKI

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The Many Faces of Communism. Edited by Morton A. Kaplan. (New York: Free Press, 1978. Pp. x + 366. $14.95.)

This book is true to its title. It includes chapters on communism in Italy (Franco Ferrarotti), France (Jean V. Poulard), Portugal and Spain (Eusebio M. Mujal-Leon), the Soviet Union (Mikhail Agursky), Eastern Europe (Teresa Rakowska-Harmstone), Yugoslavia (Nenad Popovic), Japan (Haruhiro Fukui), and China (Tang Tsou). In addition, a chapter by Pavel Machala explores the interrelationship among superpower detente, East European communism, and West European politics, while an introductory essay by Morton Kaplan examines the impact of variety in the communist world on stability in the international system. Because of this breadth of focus, the volume will serve as a useful (and somewhat unique) resource for scholars and students alike. The individual chapters are, in almost all cases, well written and of high quality.

In addition to its breadth, the volume is marked by diversity of approach to the subject matter. Some chapters deal primarily with the triangular relationship among a communist party, its domestic context, and international communism (Ferrarotti, Poulard, Mujal-Leon, Rakowska-Harmstone, and Fukui). Other chapters are concerned primarily with the broader context of superpower relationships and international relations in general (Kaplan, Agursky, Machala, Popovic, and Tsou). Some chapters rely heavily upon documentation; others provide almost none. Some take an explicitly comparative approach; others focus on a single country. Some make an effort to appeal to specialists, offering new theories or bodies of evidence (Mujal-Leon, Agursky, Machala, Fukui, and Tsou); others gear their chapters toward students, offering comprehensive overviews of existing knowledge (Ferrarotti, Poulard, Rakowska-Harmstone). Most of the contributors are successful in what they apparently set out to do. But the diversity of approaches makes the volume as a whole difficult to evaluate—if only because the criteria for evaluation would have to be inconsistent.

Some chapters deserve special mention. Mujal-Leon's comparison of the Spanish and Portuguese communist parties is an intelligent, disciplined exercise in comparative analysis. Machala's chapter is a brilliant and subtle analysis of the dynamics of European and world politics, leaving me eager to read further contributions by this new talent in the field. Fukui's chapter on the JCP is a welcome addition to the English-language literature—a comprehensive and first-rate history of JCP ideological and organizational revision. Tsou offers an innovative interpretation of Chinese foreign policy orientations, arguing that Sino-American rapprochement, and the official Chinese theory of "three worlds," reflect the studied efforts of an insightful Chinese leadership to regain the crucial middle position in the strategic triangle. The chapters by Ferrarotti and Poulard, while less innovative than those just mentioned, provide competent overviews.

All the chapters in this book have their strengths, but some have greater weaknesses than others. The essay by Rakowska-Harmstone provides a wealth of information for the interested student, but will be disappointing to the specialist. Its analytic framework is based on traditional stereotypes (e.g., *equating* "the priority of central planning" with "the primacy of the party" [p. 214]), and ignores the welfare-oriented and egalitarian social values these regimes typically promote (a mistake *not* repeated in Machala's chapter). Popovic's contribution is marred by lack of concern for documentation of controversial assertions (e.g., that the "thoroughly socialized" economy "creates a major prosocialist inertia, which in substance is opposed to the West" [p. 275]), and by assertions simply proposed as self-evident (e.g., "There is no doubt that the United States has necessarily to be involved in any issue anywhere in the world where human rights and democratic values and practices are at stake" [p. 276]).

Morton Kaplan's introductory essay should be of interest to students, scholars, and policy makers alike, but its provocative arguments and insights are tainted in several ways. First, they accept without question the proposition that "the Soviet regime is sitting on a powder keg," from which the author extrapolates likely Soviet foreign policy (p. 11). Second, they rest upon a concept of "finlandization" that has gained little currency among students of Soviet foreign policy (pp. 12, 13, 15, 16, 17, 26). Third, in an effort to appeal to policy makers, Kaplan too often reverts to normative, "in-group" terminology (e.g., that the orientations of a given CP are "dependable" or "unhelpful" [p. 17]), or advances propositions as if they were self-evident truths (pp. 24, 25).

Mikhail Agursky's chapter evokes both excitement and skepticism. In some ways it is a highly creative analysis of the history of Soviet ideology in terms of a struggle between Marxist-Leninist internationalism and "national Bolshevik" conservatism. Agursky sees the contemporary regime to be in the midst of a legitimacy crisis, "able to control its empire nly by increasing effort and cost" (p. 186). 'he consequence is an unstable "push-pull" elationship between expansionism and isola-ionism, rendering Soviet leaders both divided nd ambivalent about the desirability of foreign ommunist movements, whose rebellious orien-ations might further exacerbate the domestic oviet legitimacy crisis. The argument is dazzl-ng—in some places highly insightful—but leaves nanswered crucial questions about whether he mainstream Soviet political elite is indeed his polarized. Agursky anticipates the objec-ion: "The presence of at least two competing rends does not mean that all the members of he Soviet leadership have a polar identity. 'here is also a growing indifference toward deology as a whole. . . . But the decisive olitical role always belongs to people with an ctive identity" (p. 178). Perhaps, but how are e to classify the Brezhnev's and the Arba-ov's? Is their intermediate position less impor-ant and potentially less influential than the ositions of extremists to their far right and far eft? This issue can hardly be decided by eduction, definition, or assertion.

My status as a specialist primarily on Soviet nd East European affairs may have oversensi-ized me to weaknesses in some chapters rather han others. Readers will have to judge for hemselves. Nonetheless, this strikes me as a aluable book—useful for specialists in some reas, useful for students in others.

GEORGE W. BRESLAUER

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sterreichische Konsensdemokratie in Theorie und Praxis: Staat, Interessenverbände, Parteien und die politische Wirklichkeit. By Reinhold Knoll and Anton Mayer. (Wien, Köln, Graz: Verlag Hermann Böhlaus Nachfolger, 1976. Pp. 198. 340 Schillings; DM 52, paper.)

This is a concise and theoretical analysis of he Austrian political system. Although Rein-old Knoll and Anton Mayer have not explicit-y applied the concept of consociational de-nocracy as postulated by Arend Lijphart and erhard Lehmbruch, they have in effect dealt ith it to a large extent. A more explicit eference to the theoretical literature on con-ociational democracy would have, however, laced their findings within a broader perspec-ive.

The concentration is upon political parties, the official chambers, pressure groups, and the public sector of the economy. Throughout the book, Knoll and Mayer first deal with the constitutional and legal aspects of each topic. This approach provides the necessary background for evaluating how far the actual political behavior adheres to the legal norms.

As is true of the Basic Law of the Federal Republic of Germany, parties are barely mentioned in the Austrian constitution. They are, however, key instruments in the decision-making process. Knoll and Mayer have concluded that there are strong oligarchical and bureaucratic tendencies in the ÖVP and the SPÖ. Unfortunately, the FPÖ and the KPÖ receive no more than a few passing references. Moreover, many politicians are also chamber or pressure-group functionaries. All of this is true, but more documentation could have been provided by citing the works of Ludwig Reichhold, Kurt L. Shell, and Karl Blecha. When there is documentation, references are frequently made to studies dealing with similar developments in the German Federal Republic.

The section on the Chambers of Commerce, Workers and Employees, Agriculture, and Forestry, and the nonofficial pressure groups is the best part of the book. Their considerable influence has been clearly demonstrated. There is as well a stimulating discussion about the possibility of creating a Council of Chambers, or as a replacement for the *Bundesrat.* Knoll and Mayer also devote considerable attention to the *Sozialpartnerschaft* between business and labor, which has been an important mechanism for guaranteeing labor peace and substantial economic progress.

Much of the Austrian economy has been nationalized. During the period of the ÖVP-SPÖ coalitions (1945–1966), positions in the nationalized enterprises and the government bureaucracy were divided according to a party *Proporz* formula. While this is no longer quite the case, there is still considerable politicization in these areas. This, and the great influence of the parties, the chambers, and the pressure groups, have reduced the parliament to a rubber stamp of the decisions already reached by mainly extralegal entities. It would have been beneficial if the authors had also ventured some predictions about possible future developments of consociationalism in Austria.

On the whole, this is a solid work. A number of omissions and commissions, however, detract from its value. There is no index; too many quotations are too long, some even without footnote references. The bibliography is fairly good as far as the Austrian works are concerned, but it lacks FRG and American publications on Austrian politics.

JOHN DREIJMANIS

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Public Goods and Public Policy: Volume 3, **Comparative Political Economy and Public Policy Series.** Edited by William Loehr and Todd Sandler. (Beverly Hills, Calif.: Sage, 1978. Pp. 240. $18.50, cloth; $7.95, paper.)

In a volume which seems to offer "something for everyone," Roger Hanson notes that "the theory of public goods is appealing to the extent that . . . (1) there is a wide range of political variables that can be treated as public goods; (2) there is a large number of political problems that involve questions of the demand and supply of public goods; (3) the richness and complexity of political determinants, processes, and outcomes can be explained by abstract models that generate principles of behavior" (p. 73). The essays in this edited volume try to illustrate that the range of applications of public goods criteria to public policy questions may be quite wide: from tax questions to the rules and structures of the Olympic Games. They indicate that many questions (e.g., defense, use of natural resources, the effects of economic development assistance, etc.) can be viewed as problems of the demand and supply of public goods. But they do not indicate that the richness and complexity of politics can be explained by the abstract models.

Part of these difficulties could stem from one of the obvious objectives of the volume: to provide pedagogical guidance to the theory's implications for public policy. Thus, at least four essays are relatively "simplified" for teaching purposes. These successful pedagogical pieces include an exposition of the rudiments of the theory of public goods (by the editors), a fine illustration as to how catastrophe theory may be applied to public goods problems (by Elsie Knoer), and a discussion of simple techniques for the empirical estimation of demand for a public good (by Jon Cauley). Hanson's essay is an insightful critique of the research program relating to the understanding of politics via public goods theory.

As with all such essays, it is possible to quibble that some important items should have been included, given greater emphasis, etc. Indeed, I have such quibbles: why did the editors consider group size a dimension of analytic importance equal to that of divisibility and excludability in the analysis of public goods? Why did the important work on agenda manipulation, structurally induced equilibria and collective choice not get incorporated into Hanson's essay on understanding politics? But these are quibbles. More serious lacunae include the total absence of discussion of demand-revealing techniques and the totally inadequate exposition (for students) of the technical material in the empirical estimation article. Editorial care is important for a volume which is (even partially) pedagogical. That one of the most obvious pedagogical essays has the stylistic difficulties of Cauley's essay is most unfortunate. Much of the worthwhile substance of that essay is sure to be lost on its readership.

Although laying a pedagogical foundation for answers to Hanson's questions may be necessary, we must look elsewhere for the answers themselves. Does the remaining 60 percent of the volume indicate that the richness of politics can be explained via public goods models? Does it illustrate how a wide range of political questions are illuminated by this theory? Unfortunately, the answers are not all forthcoming in the remaining essays. First, only two are applications to "real" phenomena. DeSerpa and Happel write of the Olympics while Amacher and Tollison analyze the law-of-the-sea negotiations. And these applications are both somewhat unfortunate. Despite the authors' statement that "the 'product' of the Games is such a valuable one and the potential harm from large-scale politicization (of the games) so great. . . ," one is struck by the political frivolity of the example. And the analysis is not obviously meritorious, as it is not clear that one can achieve the authors' major objective: to apply the theory of clubs to the games so as to explain their historical success. Many reasons for this come to mind, not the least of which is the difficulty in defining the real-world analogues to the two theoretical variables: membership (in the Olympic club) and quantity of the public good the (Olympic) club supplies. The second essay, using the law-of-the-sea negotiations as an example, is less technical, more successful, and yet still not very convincing. Rather than buttress their arguments using the theory of public goods, the authors merely indicate in a very informal manner how this might be done. As such, it is suggestive, but little more.

The other four essays deal rather technically and theoretically with matters of application of the theory of public goods: the effect of "mixed (i.e., not pure) public goods" on the analysis of alliances (Stephen Schaffer); methods of analysis of environmental externalities and the calculation of optimality conditions (V. K. Smith); the analysis of optimality conditions of natural-resource exploitation and taxation to finance public goods (Morgan and Shelton); and the effect of foreign aid for developing countries on their patterns of income distribution (Thomas Crocker). Implications for public policy design are contained in the externalities article, although one wishes they had been further explicated. On the other hand, the natural resource article falls by the wayside by making much out of nothing: building a complex model by making empirically restrictive and uninteresting assumptions. This is unfortunate since it is obvious that there are many important applications of public goods theory to environmental and natural-resource questions.

The remaining two essays have difficulties of a different class. Crocker's essay is not self-contained: it uses "transaction network" and "signaling" theory without giving the reader sufficient background to comprehend (much less critically examine) the material. Shaffer's essay is unfortunate. Many other essays deal with the subjects he raises in a far less pretentious, more productive manner, and some of these have been written by the volume's editors. It is a shame that these two less successful essays cover material that is so important to political scientists. After all, the interplay between distributional questions and public policy is at the normative heart of much political science. And the real world has little to show in the way of pure public goods: almost everything is impure. Hence, the essay which deals with the analytics of such "mixed" cases is crucial to the scholar and the student. Further, this is the only essay which introduces game-theory material into the analysis; and, as again, as such it is extremely weak.

In sum, the volume provides the scholar with technical ideas which could be useful, and provides the student with a rather neat set of pedagogical essays, and a slimmer set of quality applications. As a text, it certainly should find users in public policy and political science courses. Its three strongest essays (by Hanson, Knoer, and Smith) will be very useful to all who take the time to read them. But the volume could have been far more valuable with stronger essays on mixed goods as well as on the link between public goods and distributional questions. Finally, greater care by the editors in expositional matters would have helped achieve the pedagogical objectives of the volume.

JOE A. OPPENHEIMER

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Conservative Dissidents: Dissent Within the Parliamentary Conservative Party, 1970–74. By Philip Norton. (London: Temple Smith, 1978. Pp. 331. £10.)

For many decades the British House of Commons earned the stereotype of a legislative institution that primarily debated and then legitimized policy decisions rather than being a place in which those decisions were actually made. Philip Norton's examination of British parliamentary politics from 1970–74 calls this stereotype into some question. He argues that backbench dissent within the parliamentary Conservative party increased sharply during 1970–74, that the Conservative government at the time changed many of its policies in response to political pressures from within the House of Commons, that the incidence of government defeat during the standing-committee stage increased sharply over what it had been in the 1960s and earlier, and that the government began to experience defeats on the floor due to backbench dissidence for the first time in over 30 years.

Norton gives us an invaluable description of parliamentary politics during the period. He provides not only summary statistics as to the incidence of defeats, attempts to influence government policies, and outcomes, but also excellent descriptions of the individual issues and key personalities involved. To complete the portrait, Norton presents alternative explanations as to why behavior changed in the House of Commons during the 1970–74 period and then systematically assesses the relative merits of each explanation. He finds, for example, that the whips and constituency associations (the former more than the latter) were largely ineffective in muting backbench rebellion on the floor, even rebellion that would defeat the government, unless the government's very life was at stake. The main cause of the backbench dissidence is not to be found in any of the traditional variables as much as in the stubbornly insensitive leadership of Prime Minister Heath.

Norton does not argue that permanent change in the behavior of Conservative backbenchers or in the relations between the House of Commons and the government took place during the 1970–74 period. After all, the principal reason presented for the change (Prime Minister Heath's leadership) is peculiar to the 1970–74 years. However, Norton suggests that the very behavior of Conservative backbenchers in defeating the government established precedents and perhaps new norms that conceivably could result in a permanent change of behavior.

The 1970–74 years were fascinating and perhaps critical ones for the British House of Commons. Norton has made a substantial contribution to the literature by giving us a careful and comprehensive study of parliamentary politics during these years.

JOHN E. SCHWARZ

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Ethiopia: Empire in Revolution. By Marina and David Ottaway. (New York: Africana Publishing Co., 1978. Pp. vi + 250. $22.50, cloth; $12.50, paper.)

It is extremely rare for political science research to be conducted in the midst of a revolution. It is even rarer for that research to be relatively objective, extensive and intensive, and quite sophisticated. For this exemplary study, we owe Marina and David Ottaway a great deal. Combining to advantage (for the second time—they did a major study of the Algerian revolution a decade ago) the skills of academia and journalism, they have illuminated recent Ethiopian politics as no one else has. More than this, although the Ottaways are not engaged explicitly in theory building, their research is so informed by contemporary revolutionary theory that their study will promote the integration of Ethiopian events into the ongoing task of building theory.

On the level of political reporting, the study covers the period from January, 1974 to the middle of 1977. It traces at some length the increasing mobilization of various sectors of the Ethiopian population, the imperial government's response to that mobilization, and the overthrow, like a punctuation mark in that process, of Haile Selassie by a military council, the *Derg,* in September, 1974.

It then traces the evolution of that council (in origin more a Soviet than a junta) from a body originally representative in character (if representative only of the military) to an increasingly elitist one, and from a group whose internal decision-making processes were apparently democratic to one dominated by a single man. The Ottaways document also the tendency of the *Derg* to become increasingly less reformist and more repressive.

One of the great strengths of the book is in its multi-layered approach to this political history. In part, the book is a study of the *Derg* and changes in its members, policies and processes. But that change is not treated as solely self-generated, but, at least in part, as a consequence of *Derg* response to events and processes beyond its control.

The mobilization of the population which catapulted the *Derg* to power continued after its "success." The Ottaways discuss the political evolution of various social forces: the old aristocracy, largely destroyed by the revolution; student and labor groups, initially part of or sympathetic to the anti-imperial movement, and later splitting into two groups, the smaller tentatively supporting the Provisional Military Administrative Council formed by the *Derg,* the larger, the backbone of the militant opposition to the PMAC; and finally, the peasants. In addition, the Ottaways discuss extensively the role of ethnic forces—if the Eritrean movements, Somali irredentists, increasingly conscious Oromos, and less major groups, can be so aggregated. Finally, the study describes and assesses the impact of external forces, especially American, Soviet and Cuban involvement.

Given such a complex situation, it is a marvel that one emerges from the book with the sense that the complexities have been made comprehensible, the strands woven into a reasonably coherent whole. This alone would make the book valuable. It does more, however, in attempting to answer general questions arising out of revolutionary theory: Is the revolution increasingly radical? What is the role of ideology? To what extent and by what means has the revolution been institutionalized? What is the political role of the peasants in this predominantly peasant country? What factors account for the *Derg*'s increasingly repressive treatment of its opponents? To say that the book does not fully answer these questions is less an indictment than a recognition of the immense difficulty of so doing.

The book does have its flaws. The suggestion that the revolution is increasingly radical is substantiated neither by analysis nor evidence. While one of the book's strengths is its willingness to make general statements in the absence of *definitive* evidence on important questions (what else can you do during a revolution?), the absence of evidence is nevertheless also a weakness, and extremely frustrating to the reader. For example, the Ottaways suggest that the *Derg* is meaningfully linked to peasant associations, and that the radical civilian group supporting the *Derg* won substantial nationwide support. Despite these qualms, however, this is *the* book for students of the Ethiopian revolution, and a crucially important resource for students of comparative revolutions or comparative communism.

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Soldiers and Oil: The Political Transformation of Nigeria. Edited by Keith Panter-Brick. (Totowa, N.J.: Frank Cass, 1978. Pp. xii + 375. $24.00.)

In the anarchy of academia one cannot hope that an anthology will be coherent, organized around one theme, with a lucid introduction and conclusion. If *Soldiers and Oil* disappoints, then, it is less the fault of the individual contributors and more due to weaknesses inherent in compilations of this kind. Compendia can succeed only if editors make their initial conceptions clear and insist that authors hold to it, or have organizational schemes agreed upon beforehand. As this is seemingly utopian, *Soldiers and Politics* suffers from the common lack of an overall plan and suggests individual themes rather than a consistent overview.

Lack of consistency is evident in title and contents. Although the volume is entitled *Soldiers and Oil,* only one of its five parts deals with soldiers and another with oil. The remaining three parts, though touching on these themes, are more concerned with relations between federal, state and local governments and constitutional change. The book covers a variety of subjects at different levels and by different methods. Some of the essays are historical and others statistical.

The political history of the military regime is dealt with in the first part. Valerie P. Bennett and A. H. M. Kirk raise the fundamental political questions, such as the growth of federal authority, the increase in the number of regions, and the potential political parties under military rule. Henry Bienen and Martin Fitton use survey and interview techniques to elucidate the relationships among politicians, civil servants and military officers, largely in the western region. This valuable discussion of the sharing of powers has bearing on countries other than Nigeria. One of the best essays in the book, however, is that of Ian Campbell, who is able to clarify most convincingly the complexities of change in the Nigerian military regime. M. J. Dent is somewhat more optimistic than Douglas Rimmer, another contributor, on the corrective character of Nigeria's military rule. Rimmer also discusses the political economy of the Nigerian oil industry and the impact of government participation. Terisa Turner adds an interesting essay on commercial capitalism and the 1975 coup. She points to the role of the state as an important market because of its command of vast oil incomes, and as a major buyer and regulator of the economy, controlling "opportunities to profit through commerce, politics becomes dominated by struggles for positions in the state or for access to those who have influence over governmental decisions" (p. 167). Her analysis of politics as economics shares applicability to wider areas with that of Bienen and Fitton. It is admirable in its clarity and force.

The two articles in part 3 on the creation of new states and revenue sharing in Nigeria, while important to an understanding of its politics are unexciting, though the authors, Ali D. Yahaya and S. Egire Oyobare are perhaps handicapped by the nature of the themes themselves. Part 4 contains analyses of change in local government, which is interesting to both specialists and generalists. Books on developing countries often neglect local government, despite its importance as a political arena. It is pleasant, therefore, to have these useful essays by African scholars who can bring an especial intimacy to the theme. Alex E. Gboyega and Oyeleye Oyediran give "A View from Ibadan," and Abubakar Yaya Aliyu the view "As Seen from Kaduna." Both essays point to the extension of the power of the federal government under military rule, and assess the costs and benefits this has involved when contrasted to the continuing localism. They also point to ambiguities in the jurisdictions of national, state, and local government in Nigeria under military rule.

The fifth and last part is an authoritative and lucid analysis of the work of the Constitutional Drafting Committee. Written by the editor, Keith Panter-Brick, it deals with the complex issues involved in constitution making in a society as complex as that of Nigeria. Panter-Brick also contributes an introduction, but the lack of a conclusion is to be regretted. In addition, the book has a useful set of sketch maps and selected bibliography. It is indexed.

Despite its lack of organization, the volume's individual essays could be useful to readers with special interests. The essays by Bienen and Fitton, Campbell, and Turner can be more widely recommended.

EDWARD FEIT

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Intergroup Accommodation in Plural Societies: A Selection of Conference Papers with Special Reference to the Republic of South Africa. Edited by Nic Rhoodie. (New York: St. Martin's Press, 1978. Pp. xxx + 482. $22.50.)

This is a book that should be read in context, with one's eyes wide open. If read *in vacuo* it will be at best confusing, at worst downright misleading. In 1977 in Cape Town, South Africa, a conference of 55 participants was held under the auspices of the Institute for Plural Societies of the University of Pretoria, the editor's home institution. Twenty-eight of the papers presented at the conference are collected in this volume. The conference, officially opened by the South African Minister of National Education, included a number of academics who have been active in South African public affairs. The conferees from abroad, among them such reputable scholars as the late Talcott Parsons, Walker Connor and Arend Lijphart, drew on experiences in such plural societies as Britain, the U.S., the Netherlands and Israel.

None of these circumstances by itself need undermine the scholarly value of a post-conference collection. But in fact this volume does appear to have suffered badly from the conditions which generated it in the first place. Taken as a whole, the perspective is profoundly apolitical and ahistorical.

While only about half of the relatively brief articles deal with South Africa directly, all to some extent seem informed by the authors' consciousness of presenting their papers in Cape Town in a critical period in South African history. Each in its own way implies that by understanding the accommodating formulas adopted elsewhere, sometimes successfully, sometimes glaringly unsuccessfully (e.g., Lebanon), one can not only evaluate steps taken today in South Africa but also predict the effects of alternative future reforms. That is, the book is implicitly prescriptive. But what undermines its value is that the parameters of prescriptions offered are so circumscribed and the mode of analysis so legalistic and static. For example, the state is scarcely taken into account at all. Several of the contributors do mention government policies—in Pakistan, Uganda, Syria—but without ever explicitly questioning whether relations between ethnic groups can be understood simply at the level of "intergroup" relations. The very title of the book is suggestive of the narrowness of analysis here. To analyze relations between Afrikaaners, English-speaking whites, Zulus, and others in South Africa in the 1970s without deliberate exploration of the state and its own ethnic base is rather absurd. A useful analysis of South African intergroup relations, and also those in other multiethnic societies, must directly address the role of the state and the changing capacities and bases of that state over time.

A second blind spot in the collection concerns socioeconomic class. It is not a matter of choosing between class and ethnicity in one's investigations of the sources of antagonism between sectors of a society. Rather, it is necessary to ask how class and ethnicity interact historically so as to either exacerbate or blur stratifications of power and status. Class is pertinent to analyzing intra-ethnic group relations as well. The ethnic groups touched on in most of these articles generally are treated as solid aggregations. Yet Lijphart's own concept of consociational democracy—an openly prescriptive concept—rests on the condition of sufficient intra-communal trust and allegiance within the several accommodating groups that elite members of each can be assured that they will have rank-and-file support for whatever compromises they reach. Lawrence Schlemmer, who has done valuable work on South African social change, does note that it is naive to presume that the Afrikaner whites are a united community; but he doesn't tell us any more. Without an awareness of the growing class divisions among Afrikaners themselves over the last 50 years (partly owing to the access of some more than others to the state's apparatus), it becomes impossible to comprehend the political implications of the recent "Muldergate" scandal.

The third sin of omission running throughout these articles concerns the international political economy. Once again, the South African context, of which each writer appears conscious, makes this neglect particularly puzzling. Relations among ethnic groups—and the likelihood of any policies producing genuine harmony among them—are not always and everywhere shaped by the international political economy. Yet it is a critical variable to monitor. In the current phase of world political evolution it is probably wise to start with the assumption that forces outside the given society—other states, multinational corporations, multilateral lending agencies—will indeed affect which groups have disproportionately greater access to capital and coercive force and how free they will be to use those to determine the cultural division of labor in their own society. That assumption, naturally, must be tested; but at least it should be on the initial research agenda. In the collection under review those factors hardly appear, except in Peter Janke's article, where he raises the specter of Soviet intervention in southern Africa. This omission is striking because quite a few of the contributors, according to the useful notes accompanying the articles, are or have been affiliated with organizations such as the Georgetown University Center for Strategic and International Studies. Perhaps, however, it was the conference organizers' very decision to choose so many of the participants from such security-oriented institutions that in the end produced a set of articles that makes altering inter-ethnic relations seem akin to moving pawns (and knights and kings) around on a one-dimensional chessboard.

CYNTHIA H. ENLOE

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The Fragmentary Class Structure. By K. Roberts, F. G. Cook, S. C. Clark, and Elizabeth Semeonoff. (Atlantic Highlands, N.J.: Humanities Press, 1978. Pp. viii + 200. $12.50, cloth; $6.25, paper.)

Sociologists seemingly have an insatiable appetite for studies of social class and inequality. This brief book helps whet that appetite.

The central theme of this volume is that the British class structure is changing—in fact, fragmenting. Rather than assuming the existence of a monolithic middle class, the authors argue, we need to differentiate among its various strata. The *new* middle class is the growing sector of salaried employees resulting from Britain's movement toward a post-industrial society. The *traditional* middle class is comprised of shopkeepers and the self-employed. The *proletarian* middle class is the unionized segment who identify with the working class. The *middle mass* is the modal category located in the center of the social hierarchy. Similarly, analyses of working-class voters draw distinctions among substrata of this group: proletarian, bourgeois, central working class, and so on.

Certainly many of these ideas are not new, but this work is thought-provoking in presenting such a range of subgroupings. Moreover, virtually every chapter provides a useful review of the relevant research by British sociologists. But alas, this strength of the volume in describing subgroups is also a source of weakness. The authors fail to provide a sufficiently coherent typology of class which incorporates (and justifies) the various types. Subgroups are presented in one chapter, and completely new groups might be taken up in the subsequent chapter. The reader might justifiably ask, for example, how the new middle class of chapter 6 is related to the middle-class groupings of chapter 8. There is no clear answer. The same problem hampers their discussion of working-class subgroups. To some extent this situation reflects the theoretical focus of each chapter, but the need for final synthesis is obvious.

The empirical base of this work is a 1972 survey of 474 economically active males living in Liverpool. As such, the study continues a long tradition of local surveys in British social research. While some may question the validity of such a regional sample, this criticism does not fully apply. Even a local sample can provide fertile ground for exploratory work on class imagery and class substrata. On these points, the authors' insights and analyses are valuable. However, despite disclaimers in the introduction, they also attempt to generalize from a regional cross-section sample to comment on national longitudinal trends. Their ideas on the implications of a fragmented class structure are interesting, but largely based on speculation rather than empirical evidence. If these exploratory analyses were directed to the extensive time series of British election studies, the results and conclusions might be more impressive.

Because this is an empirical study, its methodology also deserves special scrutiny. This, I fear, is the weakest part of the volume. The authors too easily dismiss the need for significance tests and measures of association—especially important with their small sample size. My rudimentary checks find them occasionally making much ado about a statistical nothing. Multivariate methods were never applied, even when the research question required them. Furthermore, indicators of concepts such as deference and class imagery were presented without validation. Even their definition of class groupings left the reader with a sense of ad hoc decision making. This research claims a scientific label, but needs more scientific rigor.

In summary, this book contains fresh ideas on old questions that many of us face. The last chapters, especially "The New Radicals" and "Politics in the Fragmentary Class Structure" are often stimulating reading. The merit of this volume is thus in convincing researchers that those crude categories of "middle class" and "working class" are insufficient for studying contemporary politics in advanced industrial societies.

RUSSELL J. DALTON

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The Soviet Procuracy and the Supervision of Administration. By Gordon B. Smith. (Alphen aan den Rijn, The Netherlands: Sijthoff and Noordhoff, 1978. Pp. xii + 154. $25.00.)

This is an important book on an aspect of Soviet law not recently given the attention it deserves. Gordon Smith, who did much of his research in the Soviet Union, was able to make use of judicial statistics and archival information not generally available. Moreover, he frequently cites conversations with Soviet lawyers, and he has obviously made good use of these conversations in making judgments as to the reliability of data and impressions gained from other sources. The bulk of Smith's data is from published sources, however, and the most important of these are 433 cases concerning the general supervision function of the Procuracy, published in the journal *Sotsialisticheskaia Zakonnost'* between 1955 and 1974. The analysis of these cases is the central focus of chapter 5 on general supervision, the key chapter in the book.

Smith considers the Procuracy the most important organ among those performing control functions in the Soviet Union, and surely this is the case. His emphasis on procuracy control, however, leads him to consider other forms of control, to the point where the title of the volume is somewhat misleading. There is an interesting chapter on court control of administration and one on citizen complaint procedure, both of which make some reference to the procuracy but have little to do with supervision. When these two chapters are taken together with the theoretical chapter 6, it can be said that something over one-third of the book has little or nothing to do with procuracy supervision.

This last-mentioned chapter, incidentally, is the least successful in the book. It contains numerous references to writings on organization theory by Western authors, but the whole discussion has at best a tenuous relationship to the data and analysis that precede it. It bears the mark of a theoretical discussion required of the author by a dissertation committee. There is other evidence of the same problem: parts of the discussion are cast in terms of hypotheses and their confirmation. This implies a level of rigor that the analysis simply has not achieved, partly because the nature of most of the data simply cannot support it. And on a couple of occasions the author goes to the trouble of calculating the statistic Tau-*C*. The correlations achieved in these cases are quite high, to be sure, but nothing meaningful is gained by the use of the statistic. Smith's work stands quite well on its own, without the use of these flourishes, which, for me at least, only detract from the impact of his analysis. Aside from the social science rhetoric, the author's writing is quite clear. There are times, however, when more explanation would have been helpful. Smith writes several times, for example, of "standing," as if some doctrine analogous to the American law of standing existed in Soviet law. Such is clearly not the case.

Smith's short concluding chapter is interesting and contains several small surprises. First, he demonstrates in the procuracy's work in recent years a "clear trend toward numerical diminution of protests expressing citizens' grievances . . . and the marked increase of representations seeking to preserve state economic interests." Further, he discusses the "primacy of the control function" (mostly involving state economic violations) over the "redress function" (largely involving citizen matters) in procuratorial supervision. This leads him to note that occasional procuratorial actions "protect state interests *at the cost of* individual interests." Along with a number of Soviet scholars, Smith believes that this makes the expansion of judicial review of administrative acts affecting citizens all the more important.

DONALD D. BARRY

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Israel: Pluralism and Conflict. By Sammy Smooha. (Berkeley: University of California Press, 1978. Pp. xviii + 462. $24.00.)

As Leo Kuper points out in his effective foreword to this excellent book, "The theory of plural societies and of divisive pluralism has generated much controversy in the brief period of its development" (p. xiii). Inspired by the work of J. S. Furnival, M. G. Smith provided the first important contemporary theoretical formulations, and Leo Kuper, Pierre van den Berghe, R. A. Schermerhorn, and now Sammy Smooha (among others) have further refined and developed variations of the theory. Smooha's work is an excellent illustration of the application of the pluralist model (not to be confused with the tradition of the same name in American political science) to an analysis of the highly complex web of intergroup relations in Israel. It is one of the best illustrations of the considerable strengths and the few disadvantages of this approach which I have read. If the reader is looking for a single sophisticated macro-sociological analysis of ethnic relations in Israel, I strongly recommend this book.

Smooha distinguishes five major plural divisions in Israeli society: the Palestinian Arabs in the occupied territories and Israeli Jews; Israeli Arabs and Jews; Druze, Christian and Muslim Arabs; Oriental and Ashkenazi Jews; and religious and non-religious Jews. From this wide range of various levels of social, cultural, economic, and political divisions he selects the divisions between Israeli Arabs and Jews, between Oriental and Ashkenazi Jews, and between religious and non-religious Jews, for close analysis. Using a typology of plural divisions, Smooha applies different models of plural relations to each of these sets. He analyzes Oriental-Ashkenazi relations in terms of a "dynamic paternalism-cooptation model," religious-non-religious relations in terms of a "contested accommodation model" (adapted from Lijphart's theory of consociationalism), and Arab-Jewish relations in terms of an "exclusionary domination model."

Smooha rejects the nation-building approach which has long dominated much of the sociological research on Israeli society. This approach views Israel as an exemplary melting pot (if not a pressure cooker) which has successfully "absorbed" the socially and culturally heterogeneous masses of immigrants and which is characterized by harmonious group relations. Among the devastating criticisms which he levels against the absorption-modernizing model are: it is ahistorical; it accepts official ideology at face value; it tends to neglect power factors; it tends to ignore the phenomenon of discrimination; and it tends to analyze Oriental-Ashkenazi relations in isolation from the other important plural divisions in the society. He is not the first to raise such objections to this approach, and unfortunately Smooha fails to give adequate treatment to alternative approaches which have been employed. He does reject the radical-left image of Israel as a white-settler, neo-colonial, racist, theocratic entity. But given the highly polemical nature of this approach, it is more of a "straw man" than a serious intellectual contender, although it does not lack vehement proponents.

Among the serious weaknesses of Smooha's formulation of the pluralist analysis of ethnicity in Israel is his rejection of what he terms "wasteful ethnographic and historicist approaches" (p. 13). Although in his own critical evaluation of pluralist theory, he makes modest claims for it, the previously mentioned rejection of the relevance of a rich and extensive anthropological literature on ethnic relations in Israel is anything but modest. Both in his critical evaluation of alternative models, and in his substantive analyses, e.g., of pluralism and inequality, Smooha justly criticizes the inadequacy of data gathered through social surveys and opinion polls, but either ignores or fails to consider adequately the wealth of anthropological studies which have been carried out in Israel. While it is true that many of these ethnographic studies fail to make macro-sociological generalizations, they could and should have been more effectively incorporated into Smooha's models and analyses.

Smooha is at his best in his application of his dynamic-pluralism-cooptation model to the analysis of Oriental-Ashkenazi relations. He shows how the Orientals were gradually coopted into the Ashkenazi-dominated system from the bottom upwards. While professing the ideals of the "ingathering and merging of exiles," the Ashkenazim looked down on the Orientals as a "generation of the desert." This unofficial paternalistic ideology has at first restricted full equality to a select few, and has delayed the acquisition of equality on a wide scale to later generations. Smooha traces the ongoing transition to a liberal competitive system where the Orientals can freely enter into competition with fellow Ashkenazim, but tend to lose out because of inferior skills and informal discrimination. This transformation marks continuing processes of weakening of ethnicity, heightening of class and power inequality, and overall improvement of Oriental-Ashkenazi relations. Ethnic calm is thus maintained by diverse mechanisms ranging from activation of Jewish solidarity and national consensus to neutralization and control.

It is impossible in such a brief review to do justice to the rich array of data and analysis contained in Smooha's impressive book. The 86 pages of tables in the appendix alone are a virtual gold mine of valuable information. Nor do I feel justified in engaging in the type of detailed scholarly criticism which would interest only specialists. In sum, this book would have considerable appeal for scholars interested in theories of social and cultural pluralism and their application to the explanation of the complexities of intergroup relations in the social scientist's ideal laboratory—Israel. It is the best and most up-to-date macro-sociological study of ethnicity in Israel, and should be essential reading for those interested in the political ramifications of such relations in Israel. It is not "light" reading, and is therefore more appropriate for upper-division undergraduate or graduate courses on the politics of ethnicity, of Israel, or of the Middle East.

MYRON J. ARONOFF

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Law and Politics: The House of Lords as a Judicial Body, 1800–1976. By Robert Stevens. (Chapel Hill: University of North Carolina Press, 1978. Pp. xviii + 701. $30.00.)

This exhaustive study of the British House of Lords contains reference to or analysis of close to 1500 legal decisions as well as commentary upon the historical and political context within which the decisions were made. It further contains a vast amount of biographical data about the lords who rendered decisions over the past 175 years, humanizing an otherwise alien judicial system. In a prolonged and painstakingly detailed account of how the original legislator in the British system developed its responsibilities as a final appeals court, even as it lost many of its legislative functions, Robert Stevens, a legal specialist, establishes beyond peradventure both his stamina and capaciousness.

In the introduction Stevens sets the stage by observing that while in 1800 the House of Lords was an equal partner, if not the senior partner, with the monarchy and the House of Commons in the governance of England, today it has a marginal effect on legislation, at best; it is a physical home for the salaried judges who form the final court of appeal, and has little of the influence it had when its constituency was the aristocracy. The forward march of democracy and utilitarianism challenged at an early date the assumptions about the inherent right of the peers to act as the final court of appeal.

Part 1 deals with the manner in which law and politics interacted to render a professionalized legal system by 1912, even though Lords remained under the influence of political Lord Chancellors, and was staffed by Lords of Appeal who were generally chosen for their political rather than professional success. The lower and intermediate appellate courts, on the other hand, were dominated by a professionalized judiciary handling doctrines that were increasingly regarded as self-contained and apolitical. While concern over the objectivity of the legal doctrines applied was voiced, as was the question whether they were derived from the logic of the common law, those who ran the House of Lords and Privy Council were aware that the appeals that reached them were not dealing with core situations but with policy issues fundamental to the outcomes reached.

Part 2 treats with the reversed situation in the subsequent phase, with enactment of the Parliament Act of 1911, Haldane's chancellorship of 1912, and the Appellate Jurisdiction Act of 1913, and extending up to the onset of the Second World War. In this phase, "the law" with its emphasis upon objectivity and logic came to be more highly regarded; judges were to emphasize the more "scientific" spirit of the English tradition. While, therefore, legislation was used increasingly as a means of nationwide social control, law—as understood by the English judges—was confined within narrower intellectual and practical bounds.

We learn, in part 3, of what Stevens calls an era of substantive formalism, ranging between the years 1940 and 1955, characterized by an urge for certainty, a stress upon literality and consistency. The implicit politics of the period was clearly one of total deference to the executive, with the value of second appeal undergoing serious attack. Legal rationality became an end in itself; the literal meaning of words was to be the only criterion of statutory interpretation. Lords, in brief, eschewed every opportunity to legislate judicially; parliamentary sovereignty was construed to mean that there should be no serious questioning of acts of Parliament or even decisions of the civil service, which arm was committed to a judge-like insistence that it was uninvolved in policy making, and might well have been the strongest force in government. Part 4 of the book spans the period between 1956 and 1976, in which years Great Britain as a world power saw decline, experienced economic truncation, and endured serious criticism of the monarchy, even as the peerage was reduced to a novelty. The law in this phase underwent demystification and the simplistic belief in substantive formalism gave way to a willingness to admit a limited interstitial legislative role in Lords, even if there has been no clear reason. Its readiness to address the rationale of the appellate process has made more probable the survival of the House of Lords, at any event.

The view that final courts of appeal are inevitably creative in their function, taken by Holmes and Cardozo, among others, in the American system, is scarcely accepted in England today. A literature abounds in America that even threatens to render extinct the doctrinal basis for adjudication, as do quantitative judicial studies, but little of this nature is to be found in the United Kingdom. Debates about judicial activism and judicial restraint, commonplace in the United States, fall short of even possessing the consciousness in England, iven the strong taboos about closely examining he work of judges, and the generally accepted iew that law and the courts play a less vital olitical and social role there. There is considerble validity to the view that the British legal ystem has changed little since the devastating ineteenth-century portrayals of marathon ases in Dickens' *Bleak House* and Trollope's *rley Farm*. It is restrictive and enamored of orm and ritual; the barrister-solicitor distincion promotes impersonality; it is oligarchic. hese are but some of the observations proided by the author and they probably help to nderstand why a royal commission is now onducting inquiry in the direction of improved egal services.

Robert Stevens' *Law and Politics* is an mpressive and vast undertaking and study of he House of Lords as a judicial body. Stevens as consulted all of the reported decisions of he House of Lords and even the unreported ecisions since 1830. Documents by, to, and bout the law lords have been consulted as well, long with their speeches, and the enrichment rovided by Stevens' extended political and sychological commentary makes this quite vident. The future of the House of Lords as a udicial body will lie in an acceptance of the lement of discretion in the final appeal proess, Stevens concludes, coupled with a responibility to articulate the purposes and goals of ifferent branches of the law. While the courts n Britain will probably never compete with the egislature, inconsistent with their political and ntellectual history as this is, some complemenarity and intelligent creativity can be hoped or. For the present, at any event, Lords as a udicial body is no longer living under threat of mminent demise and the idea that appeal udges are engaged in some form of lawmaking widely accepted politically and intellectually.

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ueens College,
ity University of New York

he Politics of Terrorism. Edited by Michael Stohl. (New York: Marcel Dekker, 1979. Pp. xiii + 419. $10.00, paper.)

The prospective editor of a collection of ssays on political terrorism has ample reason o feel intimidated. Trying to exert some entripetal force over any group of articles, all aturally flying off in different directions, is roblem enough. To make it worse, terrorism as proved a difficult subject for academic nquiry. There is little agreement on what terrorism is, much less on an explanation of causes, processes, or effects. The vagueness of the concept means that the centrifugal pull in such a collection is almost irresistibly strong. Furthermore, the controversiality of the issue, while it inspires a profusion of books, articles, reports, and conferences, often leads to emotionalism.

Despite these obstacles, this collection of analytical essays edited by Michael Stohl is a useful addition to the literature on terrorism. The articles, most of which were written for this volume by scholars new to the field of terrorism, are published here for the first time. The first section of the book contains four essays on theoretical approaches to the study of terrorism. Ted Robert Gurr presents an empirical survey of domestic terrorist campaigns in 87 countries from 1961 to 1970. Peter Grabosky considers urbanization as facilitating the resort to terrorism. Harry Targ employs a Marxist framework to argue that social structure is a major determinant of terrorism, which he finds more likely in pre-industrial and post-industrial societies than in industrial ones. Peter R. Knauss and Donald A. Strickland investigate the relationship between terrorism and the fear of anarchy, concluding that society is enormously resilient to disruption. The more extensive second part of the book, "The Practice of Political Terrorism," focuses on various geographical regions: Western Europe (Raymond R. Corrado), sub-Saharan Africa (Mary B. Welfling), Latin America (John W. Sloan), the Palestinians (Vaughn F. Bishop), Bengal (Richard C. Hula), the United States (Frederic P. Homer), and an overview of the transnational dimensions of the phenomenon (Edward Mickolus). The division of subjects is conventional, but it is hard to understand why Gurr's description is classified as a theoretical approach, while the similar survey by Mickolus of transnational terrorism world-wide from 1968 to 1975 is placed in the historical section.

Some of the individual articles are informative and provocative; the analyses by Gurr, Grabosky, Targ, Welfling, and Hula are worth singling out. However, the volume suffers from insufficient coordination. Had the essays on historical incidents of terrorism tested or at least taken into account the hypotheses proposed in the theoretical chapters, the volume's claim to theory-building would be more credible. With one exception—Welfling's comments on the relevance of Gurr's conclusions to the case of Africa—each chapter stands alone. Some of the historical essays present surveys of entire regions; other focus narrowly on single terrorist organizations. Since the theoretical virtues of the book are limited by the autonomy of the contributions, an introduction outlining the common themes of the essays instead of demolishing conventional wisdom about terrorism might have supplied the necessary unity. The readability of the volume would also have been improved by uniform citation styles and care in avoiding typographical and factual errors.

One distinctive characteristic of the collection is that it employs a concept of terrorism that includes actions both by and against the state, although only half the essays attempt to analyze both forms. The impartial use of the term "terrorism" to apply to governments as well as opponents is welcome, especially since many works on terrorism use the term pejoratively. It is also important to emphasize that governments as well as challengers may be the instigators of violence. While defining the concept broadly may be philosophically and politically desirable, in application such inclusiveness can result in imprecision and the sacrifice of depth for breadth. It is also wise to remember that government repression is not necessarily terrorism. Furthermore, the revolution-repression dichotomy can obscure the fact that revolutionaries can use repressive terrorism against their own followers.

The essays raise intriguing questions precisely because of their eclecticism. If, as Targ argues, terrorism is not likely in industrial societies, how can urbanization facilitate terrorism, as Grabosky contends? Although his data are limited, Gurr suggests that terrorism is more frequently a means of protest than of revolution, but both Targ and Welfling agree that terrorism occurs when other methods of seizing power (e.g., mass revolution or coup d'état) are unavailable. They assume, as does Stohl in the introduction, that the purposes of terrorism are either to maintain a regime or create a new one. Moreover, if the absence of other means is a source of terrorism, how does one account for the high incidence of both coups and terrorism in Latin America? Another dilemma lies in the circularity of some explanations of terrorism. In general, confusion between factors that are causes of terrorism and those that are effects seems endemic to the study of terrorism. For instance, lack of active popular support for would-be revolutionaries is considered by many, including Targ, Sloan, and Welfling, to be a condition for terrorism. At the same time, the effectiveness of terrorism as a political strategy is judged by its users' ability to gain mass support. Corrado argues that both the Provisional Irish Republican Army and the Rote Armee Fraktion in West Germany are anachronistic, because they failed to gain mass followings. It is not clear whether he means that these groups resort to terrorism because they are traditionalists opposed to modernization, or whether they are anachronistic because terrorism is inappropriate. The danger lies in inferring cause from effect.

The serious theoretical intentions of this volume distinguish it from the bulk of the literature on terrorism, much of which is policy-oriented or loosely descriptive. The book is supposed to be valuable for specialists, educated non-professionals, and undergraduate and graduate students alike. However, its complexity does not make for easy reading, and the lack of a bibliography also decreases its usefulness as a text for students.

MARTHA CRENSHAW

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Revolution from Above: Military Bureaucrats and Development in Japan, Turkey, Egypt, and Peru. By Ellen Kay Trimberger. (New Brunswick, N.J.: Transaction Books, 1978. Pp. viii + 196. $14.95.)

This interesting book attempts to develop a model of revolution from above based on an analysis of the Turkish (1923) and Japanese (1868) cases, and a brief comparative review of more recent developments in Egypt and Peru. The model is "distinct from either coup d'état or mass—bourgeois or socialist—revolution from below" (p. vii). Beginning with the assumption that revolutions from above are exceptional and unusual, Trimberger concludes from her study that the preconditions for "this type of social change are becoming increasingly prevalent in many Third World countries," and that it is mass revolutions from below that "are the truly exceptional or rare historical phenomena" (pp. vii–viii).

Stated succinctly, Trimberger's thesis is that revolution from above occurs in situations in which there is "a relatively autonomous bureaucratic state apparatus" and "a dynamically autonomous state bureaucracy." In short, control of the state apparatus is divorced from control of the economic system, or the means of production and exchange. The bureaucrats, in other words, are not members of the dominant class; nor does the dominant class necessarily control the institutions of the state. In fact, in revolutions from above, bureaucrats use their control of the state apparatus to turn against the dominant class and destroy the status quo which nurtures that class. This is most likely to occur "when there is no consolidated landed class, as in nineteenth-century Japan and Turkey, or when a landed oligarchy is in economic and political decline," and when the nascent bourgeoisie either is weak or dependent on foreign interests (p. 5). Military bureaucrats are especially well situated to undertake revolution from above because they have direct control of the coercive instruments of the state.

In the further development of her thesis, Trimberger elaborates five "necessary and sufficient" conditions for revolution from above by military bureaucrats. The first has already been stated: the autonomy of the officers from the dominant socioeconomic class. Second, the officers must develop political cohesion. Third, they must perceive a threat to their own interests in the form of penetration by foreign interests; by contrast, "military bureaucrats with ties to vested interests react to nationalist movements from below with conservative repression" (p. 42). Fourth, the international situation must be such that the military bureaucrats are capable of exploiting it for purposes of increasing national autonomy. Finally, rebellious military bureaucrats require a provincial power base from which to mount their campaign against the central government. All of these conditions are found to have prevailed in nineteenth- and early twentieth-century Japan and Turkey. In China and Prussia, by contrast, the bureaucrats were too closely tied to dominant economic interests, leading to a conservative reaction against nationalist movements rather than the forging of an alliance with them. Analysis of the Nasserite movement in Egypt and the 1968 upheaval in Peru leads Trimberger to drop the fifth of the above conditions on grounds that the prior existence of a unitary and homogeneous nation-state (in contrast to the relatively decentralized Japanese and Ottoman Turkish regimes) obviates the need for a provincial power base and a civil war against the *ancien regime.*

Given this pessimistic conclusion, one wonders about both the applicability and the utility of the model set forth here. If revolutions from above fail to break the stranglehold of international capitalism, are they to be preferred to any other alternative? For that matter, is any significant improvement in social conditions in Third World countries possible without a major upheaval in the prevailing international political and economic structure? Unfortunately, larger questions such as these are not explored in this book, save for suggestions in the last two pages that only relatively populous Third World countries with large potential internal markets and "strong and independent mass socialist or communist" movements capable of mobilizing the masses for the "prodigious human effort necessary to achieve autonomous industrialization" can in the long run succeed.

It must also be noted that this laudable effort is marred by the publisher's allowing far too many glaring errors to slip through the production process, including in particular a crucial garbled sentence at the bottom of p. 5. The book might also have benefited had more recent literature been included (very few items published after 1973 are included in the bibliography).

In sum, this is an interesting essay, although in important respects it remains a truncated effort.

FRANK TACHAU

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Social Deviance in Eastern Europe. Edited by Ivan Volgyes. (Boulder, Colo.: Westview Press, 1978. Pp. xv + 198. $16.00.)

This collection includes eight separate articles, each with its own understanding of "social deviance," and each with unique questions and approaches. Not only is the book *not* integrated, not even cosmetically by an introduction or conclusion, but also the authors individually make little attempt at comparing patterns of deviance and control among East European countries or between them and other countries. When an occasional cross-national difference does emerge, the authors rarely attempt to explain it. All the same, many of the articles are worthwhile, and most of them will interest students of East European politics and society.

The book contains one theoretical essay (Shapiro); three characterizations of deviance in a single country—Romania (Gilberg), Yugoslavia (Klein), Czechoslovakia (Ulč); two depictions of the private sector of the economy—in Hungary (Volgyes) and in Poland (Korbonski); and two studies of particular forms of deviance—alcoholism in five countries (Kerr) and prostitution in Hungary (Volgyes and Peters).

The opening and most thought-provoking contribution is the theoretical article by Paul Shapiro. The author succeeds in demonstrating the utility for the study of deviance in Eastern Europe of some distinctions made by Robert K. Merton in his 1938 article "Social Structure and Anomie." A line of argument which emerges from Shapiro's analysis is that the exclusive and small number of definers of what is officially deviant in most East European societies makes unquestioned legitimacy of their rule a critical precondition for social acceptance of the norms they generate. Fearing the absence of such legitimacy, political leaders in Eastern Europe tend to overcriminalize, that is, to treat most or all devaince as aberrant and little or none of it as merely nonconforming. As engaging as these hypotheses are, they are not addressed by the authors of the more empirical contributions.

The three studies of deviance in a single country differ so much in character that one cannot try to make comparisons. Trond Gilberg pictures the problem of social deviance in Romania as the result of the attempt by a puritanical modernizing leadership to impose its standards upon a premodern, rural population. Gilberg goes on to use the official ideals of political, economic, social, and cultural behavior (which he derives from Ceasescu's speeches) as the norm against which to judge the amount and significance of devaince in Romania. Whether Romanian authorities take themselves so seriously and treat low productivity, political apathy, and divorce as deviant, I am not at all sure. George Klein offers a fine account not of deviance in Yugoslavia but of the major trends in the social structure of that country which might help one to understand deviance there. Unfortunately, Klein only alludes to features of Yugoslav deviance itself, such as the absence of economic crime in the socialist sense because of a different law and the disruptive effects of the migration of workers to Germany. In his study of deviance in Czechoslovakia Otto Ulč also pays little attention to actual trends in deviance or control, offering instead a series of analytical distinctions of variable quality (deviant versus criminal; political versus ordinary criminality; normal versus pseudo-deviance). One does learn *inter alia* that since the traumatic Soviet invation in 1968 corruption and the use of drugs have markedly increased.

The two interesting articles on the private sector of the economy in Hungary and in Poland deal more with legal than with illegal activities. Volgyes goes on to describe the recent increase in Hungary of "bakshish" culture (tips, honoraria, bribes). Andrej Korbonski shows how the presence of the largest legal private sector in Poland has in no way stunted the growth of an illegal one. Apparently, the "gap in the assortment of goods and services provided by the state" is large enough to support both.

The studies on particular crimes yield limited insights. J. L. Kerr briefly describes the varying patterns of alcohol use, abuse, and control in five countries. He concludes that the authoritarian regimes in Eastern Europe have attacked their alcoholism problems no more vigorously than Western states, partly because of a sensitivity to public opinion and partly because of the economic revenue at stake. From Ivan Volgyes and John Peters one learns that in relatively tolerant Hungary three types of prostitute have emerged, each serving a different client—workers, upper-class clients, and Italian tourists.

PETER H. SOLOMON, JR.

University of Toronto

Egypt: Burdens of the Past, Options for the Future. By John Waterbury. (Bloomington and London: Indiana University Press, 1978. Pp. vii + 313. $19.50, cloth; $9.95, paper.)

John Waterbury is a skilled writer and an astute observer of politics in North Africa. *Egypt: Burdens of the Past, Options for the Future* reflects both attributes. The book is a compilation of 16 American Universities Field Staff reports prepared by Waterbury between 1973 and 1977. The reports are organized around seven interrelated themes: guns and/or butter; coping with population growth and fertility control; agricultural versus industrial development; socialism or state capitalism versus private enterprise; authoritarianism versus controlled liberalism; Arabism versus Egypt-firstism; positive neutrality versus alignment with the West.

While suffering some minor repetition, the book is far more than a reader. The articles fall in time sequence, with the later articles building upon information presented in the earlier ones. Collectively they take the reader on a Cook's tour of the myriad factors that have rendered the political, social and economic development of Egypt seemingly impossible. Stops along the tour include before-and-after analyses of the October 1973 war, a trenchant analysis of the population explosion which has seen the Egyptian population grow from less than 3,000,000 at the end of the eighteenth century to a projected 60,000,000 in the year 2000, innumerable examples of bureaucratic ineptitude, equally innumerable examples of the misapplication of socialist principles, the imponderable problems of municipal government and management in Cairo, the intellectual debates surrounding Sadat's cautious program of de-Naserization, the shift to a mixed public/private economy, and the multitude of complex problems relating to land-water management. Along the way the reader is depressed by the plight of Egypt's poor and yet entertained by the plight of Cairo's bus system.

The *most impressive aspect of the book, in* my view, is the detailed portrayal of Sadat's persistent movement toward peace with Israel and rapprochement with the United States. The key to both, as well as to the initiation of a new economic policy, in Waterbury's view, was the Egyptian success in the October War.

> The crossing of the Suez Canal was a necessary although not sufficient condition for moving ahead on issues of economic liberalization, foreign investment, and political reform. It established Sadat as a national hero, and having acquired his own image and stature, independent of his association with Gamal Abdul Nasser, he could afford to advocate and implement new policy initiatives. Of equal importance was the fact that the October War restored the Egyptian army to the role of a conventional military force rather than the collection of officer-led cliques that had involved themselves so intimately in domestic politics under Nasser.

Once Sadat had established himself as his own man, according to Waterbury, it then became possible for him to approach the question of peace with Israel and rapprochement with the United States. Peace with Israel was viewed by Sadat as being the only means of restoring the viability of the Egyptian economy. Egypt had to choose between guns and butter. The country could not afford both. Rapprochement with the United States, in turn, was perceived by Sadat as a prerequisite for peace with Israel. In Sadat's view, according to Waterbury, "Only the United States, because it has consistently underwritten Israel's military superiority and economic viability, could cajole or push the Israelis toward compromise."

The political and economic logic for rapprochement with the U.S. and peace with Israel contained in Waterbury's analysis is compelling. Had the book been published a year or two earlier (when the last of the reports were published), Sadat's peace initiatives would have come as far less of a surprise.

In methodological terms, the various reports are a mixed bag. All reflect Waterbury's five years of residence in Egypt. Some rely heavily upon the Egyptian press and upon interviews with leading officials. Others, such as the analysis of Egypt's land and water resources, rely upon a wealth of detailed economic analyses and feasibility studies. Having lived in Egypt for a year, I can offer the observation that Waterbury's descriptions and analyses ring true. In terms of a value orientation, it is clear that Waterbury likes Egypt and the Egyptians. He takes little delight in the hopelessness he describes.

MONTE PALMER

Florida State University

India at the Polls: The Parliamentary Elections of 1977. By Myron Weiner. (Washington, D.C.: American Enterprise Institute, 1978. Pp. 150. $3.75, paper.)

The Indian parliamentary election of 1977 was an event well worth political scientists' attention. Called by Indira Gandhi at a time when her own dictatorial power seemed secure, it astonished those who follow Indian politics. Observers of political change in the developing nations must have been taken similarly by surprise. Elections terminated by dictatorships have been common enough in poor, heterogeneous ex-colonies; India demonstrated the opposite.

Myron Weiner was enterprising enough to get to India a few weeks before that dramatic event. He visited five major cities during the campaign. He talked with candidates, party workers and the newly unmuzzled newspaper reporters for whom, of course, the defeat of Mrs. Gandhi at the polls was the story of a lifetime. This information, together with the election returns, including follow-up state elections during the next year, were the raw material for this book.

The first half of the book simply tells what happened. Weiner quickly supplies the background for Mrs. Gandhi's declaration of emergency on June 26, 1975, and summarizes its oppressions and accomplishments. He joins those who have speculated as to Mrs. Gandhi's reasons for submitting her regime to elections, but without making that decision seem quite rational. Certainly, he points out, having silenced critics, she lacked feedback as to popular resentment toward the more brutal uses of government power during the emergency. Weiner makes good use of newspaper reports and campaign rhetoric to convey a sharp picture of that resentment. For example, he quotes a Muslim notable, the *imam* of Delhi's great mosque, explaining during the campaign his willingness to support militant Hindu activists against Mrs. Gandhi:

> It was not the RSS or the Jana Sangh that fired on Muslims at Turkman Gate, but the government. It was not the RSS and Jana Sangh that introduced coercion into family planning, but

> the government.... Freedom for Muslims depends not upon the election of a pro-Muslim government, but on living in a country in which the laws protect all communities.

Appendices supply the texts of party platforms and other relevant documents. Of two states whose election politics were not dominated by resentment against government excesses—Tamil Nadu and West Bengal—Weiner presents short sketches based on his observations. I must confess that his account of Tamil Nadu left me still puzzled as to the lack of reaction to the unseating by Mrs. Gandhi a year earlier of the admittedly corrupt but nevertheless popularly elected state government.

The second half of the book is an analysis of the election returns. The reader can find here a record of the party vote by states, comparisons with the last previous election (1971), and comparisons of certain categories of constituencies with the general vote by states. Weiner uses this last form of cross-tabulation to get at the question whether some groups of voters turned against Mrs. Gandhi more strongly than others. Explanations of voting behavior from aggregate data (survey research is well developed in India, but in 1977 Mrs. Gandhi surprised the pollsters as well as the rest of us) is, of course, vulnerable at best. The applications of it here are of uneven quality. Most valuable, to me, was Weiner's showing that constituencies reserved for candidates of Scheduled Castes (ex-Untouchables) not only turned against Mrs. Gandhi at least as strongly as the rest of the electorate did, but also generated a sharper increase in turnout in 1977 than did their states taken as wholes. This phenomenon was confined to the northern states where Mrs. Gandhi's son, Sanjay, bullied the bureaucracy into forcing sterilizations and bulldozing squatter hutments. Since it is now accepted that the lowest strata of society bore the brunt of these brutalities, Weiner can thus offer circumstantial evidence that the election provided a weapon of defense to those who needed it most, and they used it.

Upon other possible explanations of the vote the evidence is not so cogently marshaled. Weiner does not attempt to disentangle the two most plausible explanations for the dramatic regional disparity of the returns. Indira Gandhi's Congress won only two seats in the Hindi-speaking North; the victorious Janata won only six seats in the Dravidian-language South. Was it because, as we just saw, people in the North were abused more during the Emergency? Or because the former opposition parties which banded together to create the Janata party (Weiner helpfully provides previous election data aggregated for these parties) ha their long-standing support-bases in the North The two explanations are almost but not quit congruent; the returns could have been manipu lated to do some testing. Likewise, a closer loo at returns in some heavily Muslim constitu encies would have tested, at least crudely Weiner's very plausible proposition that Mus lims defected from the Congress even mor massively than others.

This book is probably, therefore, not the las word in the scholarly interpretation of th meaning of the 1977 elections. Weiner himsel may push analysis further, as he has done fo previous elections. Meantime, it is a worth increment to the American Enterprise Institute's very useful series of worldwide electio reports. In addition, it is a clear, authoritative and evocative lesson in the value of the ballo to those who most need it, as distinguishe from those most accustomed to it.

HENRY C. HART

University of Wisconsin, Madison

Pressure Politics in Contemporary Britain. By Graham Wootton. (Lexington, Mass.: Lexington Books, 1978. Pp. xii + 256. $19.95.)

Graham Wootton has been one of the mos prolific writers on the place of pressure group in British politics in this century. His lates book, which draws upon events from th mid-sixties to the late seventies, attempts t assay the extent to which the new position o such groups gives the British system increasingly corporatist features or leads to the condition that Samuel Beer has called pluralist stagnation. His conclusions are mixed and tentative. Fo him, as for most students of pressure politics, good data and ready answers are not easy to come by.

Wootton does not employ grand theory or especially intricate methods, although he does go beyond simple description. Because of the vast number of politically active groups in Britain, ranging from the lovably eccentric and bizarre ones dear to Anglophiles to the militant unions so unpopular with recent voters, the need for classification and selection is apparent if generalizations are to be soundly based. Wootton finds previous attempts at coining workable classifications unsatisfactory, and introduces his own, a fourfold typology based on the degree of groups' political specialization and the degree of openness of their membership. He proceeds to put into its cells a selection of great and small groups, whose pressure activities he chronicles in the body of the book, which is rich in political detail and mainly narrative in form. This rather modest effort at constructing a typology grows out of good intentions; but its theoretical basis, or the improved power it provides, are far from self-evident, and the choice of the mini-cases to carry the burden of each type of group, though presumably defensible "on the basis of apparent political significance," ties the generalizations mainly to well-reported recent controversies such as the third London airport and commercial radio.

This framework does enable Wootton to gain a basis for judging the validity of the pluralist stagnation thesis, however. Even though his findings understandably are based on a small fraction of recent pressure group efforts, they are sufficient to lead him to conclude that Britain is neither gripped by a paralyzing inability to change policy in many areas nor on the brink of Mussolini-style corporatism. He shows that groups of all of his types have both succeeded and failed in the pursuit of their interests, by a growing variety of tactics. Although they induce a healthy skepticism over the position which elevates private group power to the center of the British political system, Wootton's efforts do not put to rest completely the concern of other commentators that group power has been exacerbated by failures of structure and will in the traditional governmental center.

The strengths of this book lie in the questions it raises about trends in British interest group involvements. its focus on outcomes and effects beyond mere process matters, its up-to-date selection of material, and its combination of graphic detail and sensible assessments. It is also written with verve and wit, though occasionally with a surfeit of word play. The book does not add notably to interest group theory, nor does it purport to give a comprehensive tour (in less than 200 pages of text) of group life in British society or the range of its links with political institutions. But it does convey a strong flavor of the newer issues which pressure groups have helped put on the agenda of British decision makers in the past 15 years.

Britain remains a valuable site for exploring, from both analytic and normative perspectives, changes in the evolution of the conventional distinction between the public and the private realms; and pressure group behavior provides one key to the tougher dilemmas which have emerged. To that end, Wootton's insightful, if impressionistic, book provides a useful stocktaking of where we are, and how far we still need to go.

JAMES B. CHRISTOPH

Indiana University

International Politics

The Foreign Policies of African States. Edited by Olajide Aluko. (London: Hodder and Stoughton, 1977; Atlantic Highlands, N.J.: Humanities Press. Pp. vi + 243. $9.00, paper.)

Olajide Aluko of Nigeria has attempted to cover the foreign policies of a vast array of African states in this volume. In 12 chapters—three of which the editor prepared himself—scholars deal with Algeria, Egypt, Ethiopia, Ghana, Guinea, Ivory Coast, Kenya, Nigeria, Tanzania, Zambia, and Zaire. Aluko's introduction is called "The Determinants of the Foreign Policies of African States."

Aluko deserves much credit for assembling essays on a representative sample of countries, particularly when he has been able to recruit writers such as Boutros Boutros-Ghali of Egypt and Thomas Kanza of Zaire who have played important roles in their own countries' foreign policies. A chapter on Libya or Sudan would have been welcome, but an editor has to set the limits somewhere.

All essays are clear descriptive surveys which seem to have been written in 1972 or mid-1973 with some last-minute update work here and there. In his introduction Aluko explains domestic and extra-African influences on foreign policy through the 1960s. While the categories—economic, political, colonial heritage, ideology, geography, and cold war environment—are useful, the examples are quite dated: Aluko points, for example, to the now-defunct East African Community as the most successful attempt at economic cooperation ignoring more recent efforts such as the Economic Community of West African States. And, the statement that African states "have very little to fear from each other" (p. 12) rings ironically these days. Chapters are arranged alphabetically by name of country rather than by analytical categories.

Boutros Boutros-Ghali's essay is a five-page outline dealing with the historical continuities in Egyptian policy, but there is no analysis of the country's important break with the Russians. Negussay Ayele's piece on Ethiopia is really about Haile Selassie's policy. 'Ladipo Adamolekun provides his usual sympathetic account of Sékou Touré's idiosyncratic and hypocritical foreign policy.

On the other hand, John Akumu's chapter on Kenya, Aluko's chapter on Nigeria, and Timothy Shaw's contribution on Zambia are good for their analyses of basic problems and issues, although they, too, are dated. Okumu gives a detailed explanation of Kenyan relations with Somalia and other neighbors. He explains Kenya's strategic position and presents useful (although old) data. Aluko describes changes in Nigerian foreign policy beginning in 1970 with respect to its immediate neighbors, Africa in general, and the non-alignment movement. He shows how policy makers are trying to increase economic ties with the rest of Africa and deals with the new concerns of Nigeria in southern Africa although a discussion of Angola is conspicuously absent.

Shaw's contribution is refreshing because of his brisk application of class and elite theory to Zambia. He examines the role of multinational corporations, Kenneth Kaunda's personality, and the importance of elites' efforts to protect their status. In his view, the most important determinant in Zambian policy making is the ruling classes' desire to avoid "a radical restructuring of the political economy of southern Africa" which would loosen their own internal control (p. 231). Even though one might not agree with Shaw, he supplies a provocative framework for viewing foreign policy in Africa.

Because of the absence of other similarly comprehensive texts, this book could serve as an introduction to African foreign policies in undergraduate courses on Africa or in general courses on foreign policy. The absence of a bibliography and index restrict its utility for this purpose, however.

BRIAN WEINSTEIN

Howard University

Romanian Foreign Policy Since 1965: The Political and Military Limits of Autonomy. By Aurel Braun. (New York: Praeger, 1978). Pp. xii + 217. $18.95.)

In discussing the extraordinary course of Romanian foreign policy over the past two decades, students of the subject typically address themselves to two questions: (1) Why have the Romanians engaged in this consistently delicate and often dangerous challenge to Soviet dominance? and (2) How have they been able to get away with it? Considerations of the subject have not always been careful to separate these two aspects, often emerging as a confusing mixture of necessary and sufficient factors. The strength of Aurel Braun's book is that it does not fall victim to such confusion. Braun addresses himself clearly to the second of these questions, and he remains true to his focus throughout.

This is, however, also a source of weakness in the volume. Because Braun sticks so loyally to his central question—a delineation of the "defenses" the Romanians have been able to use since 1965 to protect their foreign policy—he never touches the major question that fairly screams to be answered, i.e., what are the Romanians up to? What is the purpose of this policy? What goals is this policy aimed at fulfilling and/or what stimuli, internal and external, drive this policy through those twists and turns that Braun describes? These questions are not addressed. While this is not the main purpose of the book, such questions are clearly germane, even crucial, to the volume's stated subject.

Thus, the book's title is somewhat misleading; for what this book is *not* is a study of Romanian foreign policy since 1965. It is, rather, an analytic essay, carefully constructed, well organized and amply documented (though rather thin on previous Western works on the subject). Braun examines the "active defenses" of Romania's autonomous foreign policy, i.e., those skillfully wielded by Bucharest, and the "passive defenses," those inherent in Soviet policy which presumably have allowed a degree of nonconformity to emerge or even flourish. The reader is best advised to come to this work already well informed on the specifics of Romanian foreign policy, pre- and post-1965 (especially the former), in order to be able to learn from it. Except for an occasional reference, for example, Braun does not discuss the Gheorghiu-Dej period. This leaves some rather striking lacunae, such as the almost complete absence of discussion of the 1964 Romanian Workers' Party Statement, a bedrock document in the development of Romanian foreign policy.

Regarding the period under study, Braun does make several interesting, if somewhat narrowly drawn, points: for example, his discussion of the Soviet view of international law and its relationship to relations among socialist states, and his attempt to sketch out how various Soviet military leaders might view nonconformity in East Europe. But in both these cases and several others, Braun either fails to demonstrate the impact of such factors or fails to gauge that impact accurately. His consideration of the role of China, for example, overstates the economic impact of that state on the Romanian situation, understates its political aspects (in contrast see, for example, Jacques Levesque, *Le Conflit sino-sovietique et l'Europe de l'Est,* l'Université de Montreal, 1970) and offers no conclusive assessment at all as to its military aspects. A more serious failing lies in the section dealing with Romania's "political/ideological defenses." Here very little attention is paid to the Romanians' decade-long attempt to redefine themselves as a socialist developing country and to seek support from nonaligned and Third-World countries—as well as increased aid from developed states—on that basis.

The book's best chapter deals with the Romanian capacity to offer "active military defense of foreign policy autonomy," i.e., to deter a possible Soviet intervention and defend the country should such an attack take place. Braun does not confine himself to a mere weapons-and-forces calculation, but explores also questions of preparation against surprise, overall morale, party control and the use of irregular forces.

The book is written in a somewhat self-conscious tone and is plagued by several disturbing contradictions: e.g., Soviet foreign policy is described as "not *per se* interventionist" (p. 190) but also as "paranoid about controlling the bloc and ensuring strict cohesion" (p. 195); periods of Soviet military maneuvers are labeled merely "intimidation" at one point but as "crisis years" further on.

Finally, while anyone studying Romanian foreign policy is bound to be impressed by the skill with which the various situations and issues have been handled by Bucharest, to view the evolution of this policy over 20 years as "a very complex set of defenses that had to be coordinated carefully in order to provide an effective defense network that would have allowed the country to pursue its autonomous foreign policy" (p. 200) makes too many of this mistakes associated with rational-actor models and makes the greater error of mistaking for an end in itself a foreign policy which is in fact a means to an end, an end which in this work remains unexplored.

RONALD H. LINDEN

University of Pittsburgh

Secession: The Legitimacy of Self-Determination. By Lee C. Buchheit. (New Haven and London: Yale University Press, 1978. Pp. xi + 260. $17.50.)

The overlapping claims of domestic and international law were never clearer than in the case of secession by a minority from a legally constituted state. The contest is highly charged as both protagonists invoke morality, international law and broken promises and usually involve outside states and international organizations in their dispute. The problem of secession by a group of people who occupy a particular territory in a legally constituted state, can be considered in several ways, none of which provides complete and satisfactory answers. It can be examined as a domestic national issue, as an international question, or as both. Lee Buchheit touches upon all three aspects of the problem but chooses to focus upon "the implications for the international community of appeals to the principle of self-determination by secessionist groups within independent states." It is within this context that the following remarks are made.

At the heart of the problem of secession is the doctrine of self-determination. As was noted by scholars during the early post-World War I period—when the old order of multinational states and empires was being replaced by new national states—it was not self-evident either as to how these states could be constructed so that they included only all, or nearly all, the "national" political unit which seeks to establish a state of its own, or what to do with minorities who are physically trapped within these new states. If the doctrine of self-determination were carried to its extreme, there would be no group too small or territory too insignificant to be denied the benefits it implied.

After World War II, the victorious states, through their promulgation of the United Nations' Charter, committed themselves to—among other things—the end of colonialism. In the first article of that document they declared as one of their purposes to "develop friendly relations among nations based on respect for the principle of equal rights and self-determination of peoples." Buchheit points out, in a succinct discussion of the drafting of that article, that the representatives of the nations which were drawing up the charter were not in agreement in their use of the terms "states," "nations," and "people," and, more importantly, on the question of whether or not the right of self-determination implied the right of a people to secede from an existing state. Earlier, he commented that "no State's population is so homogeneous that its leaders may openly embrace the right of [self-determination] without some lingering uneasiness. Nor could the United Nations (whose membership, interestingly, is limited to states—not nations or people) realistically be expected to advocate a principle that might operate to dismember the territory of its members." Buchheit notes the inconsistency between justifying on moral and legal grounds the desire of a colonial people to cast off the domination of their foreign governors and denying the plea of a minority which feels itself imprisoned in a state and governed by an alien majority of an identical means of relief.

In contemporary terms, Buchheit takes particular note of the interference or non-interference of outside forces on behalf of both the legal group and its secessionist opponents. He examines legal precedents and new legal doctrines which have been used, both to support and to deny foreign intrusion into "domestic matters." In a careful review of the Congo issue of the early 1960s, of the Kurds' attempted secession from Iraq between 1961 and 1975, of the tragedy of Biafra, of the problem of Somali people in the relations between Kenya, Somalia and Ethiopia, of the unending struggle of the Nagas from India and, finally, of the secession of Bangla Desh from Pakistan, Buchheit searches inductively for the standards of secessionist legitimacy which transcend the particular cases and can be raised to general principles for use in settling future secessionist cases without resorting to war and interference in domestic matters by the military forces of the UN or of independent nations. No clear principles or guidelines emerge for modifying international law so that it can speak with authority on the issue of secession. As the author makes plain in several places throughout his study, the issues which provoke the demands for secession are internal and so long as international law deals mainly with interstate relations it can offer no consistent guidelines for solving secessionist questions.

To conclude his study, Buchheit attempts to develop what he calls a standard of legitimacy, which, if it were adopted and well publicized, as well as subscribed to by states, might provide a means for adjudicating secessionist disputes without seeing them resolved by force of arms. Admittedly, his proposal is vague and imprecise; however, it moves the argument away from considering each case by different standards and relies instead upon creating new law which will take account both of the state's right to maintain its territorial integrity and the secessionist's claim to self-determination. Buchheit's argument is clear and reasonable, and it makes a good beginning. Whether it will cause legalists and politicians to join efforts to find a peaceful way to resolve this important question remains to be seen.

This is an important study which both traces and summarizes the existing literature while at the same time commenting upon it. It seeks to carry us beyond the quagmire of particularism into a discussion of the issue of secession against a universal standard which provides a possible way out. It is well written, tightly reasoned and a pleasure to read. One hopes that the audience for whom this superb book was intended—legal scholars, states and their discontented minorities—will read and profit from it.

JOSEF SILVERSTEIN

New Brunswick, New Jersey

Power Politics. By Martin Wight. Edited by Hedley Bull and Carsten Holbraad. (New York: Holmes and Meier, 1978. Pp. viii + 317. $22.50.)

The original version of Martin Wight's *Power Politics* was published by the Royal Institute of International Affairs in 1946. An essay of 68 pages, it has had a deep influence on the study of international relations in the universities of Britain. The work being reviewed is based on Wight's revision and expansion of this essay during the 20 years of scholarly activity that preceded his death in 1972. Wight's unfinished manuscript was completed for publication by the editors, whose scholarship is evident in the book's intellectual coherence and integrated style. The editors' introduction is itself an excellent critique of the work. I shall gladly acknowledge their judgments as I develop my own evaluation.

Two generations have passed since E. H. Carr noted that the science of international politics was in its infancy (*The Twenty Years' Crisis*, London: Macmillan, 1939). The lively polemics that have followed between traditionalists and behaviorists have undoubtedly advanced the study of international relations by diversifying the approaches to theory. But no truly definitive works have been written nevertheless. Thus, the value of a scholar's contribution may be assessed better by the questions raised in the field than by the answers provided. For modern science insists upon the hypothetical nature of empirical knowledge. Consequently, those works are classics of international relations whose postulates raise lasting and vigorous controversy. One remembers Hans Morgenau's *Politics Among Nations* (New York: nopf, 1948), and Morton Kaplan's *System d Process in International Politics* (New ork: Wiley, 1957). As Imre Lakatos points ut in his "Falsification and the Methodology f Scientific Research Programs" (in Imre akatos and Alan Musgrave [eds.], *Criticism d the Growth of Knowledge,* Cambridge, 976), not an isolated theory, but only a series f theories can be said to be scientific: to apply e term "scientific" to one single theory is a category mistake" (p. 119). Martin Wight's *ower Politics* is in the mold of such classics. It also unqualifiedly of the traditionalist hool. On the other hand, it does not present a eneral theory of international relations based n power. As the editors note, it provides no ethodological prolegomena, but addresses itlf directly to the substance of international olitics.

A substantial portion of the book addresses ich fundamental features of international poli- cs as dominant powers, great powers, interna- onal revolutions, vital national interests and restige, the balance of power, war and inter- ention, and international anarchy. Wight's ialysis of these factors is grounded in a iorough and sensitive knowledge of history, re in contemporary literature. It combines isight with erudition, clarity of language with chness of illustration.

But is Martin Wight's *Power Politics* a curio: ell done, but useless to today's concerns in e study of international politics? In agree- ent with the editors, I must stress that Wight akes little or no attempt to relate to the ademic literature on international relations at has engulfed the field during the 20 years labored to expand the original essay. Given is approach, it may be understandable why he iose not to come to terms with the issues on ethodology raised by the social scientific hool. His indifference to the economic pects of relations among states; the role layed by transnational actors other than ates, like classes, political parties, or multina- onal corporations; or the importance of Third orld countries in reshaping the international stem is, on the other hand, more surprising. It etracts from the value of the book all the ore because the structure of Wight's approach ould have easily incorporated consideration of iese aspects of contemporary international lations.

Wight did attempt to up-date his analysis in e important aspect. There are chapters on the ms race, disarmament, and arms control. hese chapters are, however, less insightful and ttle developed conceptually, and suggest that Wight had not fully understood the unprecedented and potentially severely disruptive impact of the military technology of the nuclear era. Lest I appear too severe in this judgment, I would emphasize that this is a fault widely shared with the authors of the conceptual literature on international politics, regardless of methodological approach. The prolific literature on deterrence, nuclear strategy and arms control, and the wide-ranging literature on international politics have not been integrated into conceptualizations that articulate the crucial roles of nuclear technology in international politics.

Power Politics, after all, focuses on the elements of continuity, not change, in international politics. This is the main strength of Wight's work and its greatest utility for contemporary scholars. Wight's study is a rich source of insights and verbal models providing working hypotheses worthy of test by theorists of international relations—regardless of methodological approach.

CIRO ELLIOTT ZOPPO

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The German Problem Reconsidered: Germany and the World Order, 1870 to the Present. By David Calleo. (Cambridge: Cambridge University Press, 1978. Pp. xi + 239. $11.95.)

What is the German problem? Ought the German problem be reconsidered? Does the German problem need reformulation in 1980? David Calleo, in his elegant attempt to look at old material in a new and interesting way, has attempted to answer these questions by examining Germany and the world order from 1870 to the present.

Calleo's thesis is that Germany's history of aggression should be regarded primarily within the context of the evolution of the European nation-states. By treating internal German politics only superficially he implicitly rejects the thesis (espoused by Bracher and Dahrendorf, among others) that blames German aggression on the allegedly vicious nature of the German political culture. Calleo argues that Germany's behavior as an aggressor in the international system is not different from what he describes as the similarly expansive behavior of the other major nation-states. At the same time he also plays down the uniqueness of Germany's domestic political culture, and particularly objects to the generally accepted thesis that Germany was uniquely aggressive externally because she was uniquely vicious domestically. His evidence is not convincing. Perhaps Germany was imperialist in a sea of imperialism, yet the existence of the Prussian three-class franchise and the existence of an imperialistic foreign policy were not unconnected. Unresolved domestic problems prescribed an aggressive stance abroad. Although Calleo may be right that Germany's response to modernization was not basically different from that of other countries, it went further than that of its neighbors. The restless, expansionist character of German foreign policy mirrored internal tensions.

Calleo argues for historical continuity in German politics. Although he does not specifically treat the current German controversy of the 1945 *Nullpunkt,* he spends much time discussing intellectual and geopolitical constants in German history. The omission of a discussion of internal politics again flaws the argument. The historical continuity in German politics lies primarily in the internal political and social structure. The failed revolution of 1848 and the outlawing of the Social Democratic party in 1878 contributed to the lack of trust in democratic institutions and fear of the left characteristic of the German bourgeoisie. Even today these fears surface in the polarization of parliamentary debates. Yet at the same time there are breaks in the continuity of German history, particularly after 1945. The experience with Nazism, the 1945 collapse, and the experience with Communism next door have created a new international situation. The modernization of German society, the American and European influences, the new social and economic mobility have created an atmosphere in which the older ideological discussions which figure so prominently in this book have lost much of their relevance. The German problems today are much the same as those faced by the rest of the industrialized countries of Europe.

In his revisionist attempt to play down the uniqueness of Germany, Calleo devotes only a paragraph or two to the Weimar Republic. The dichotomy between cultural flowering and political disorder, the contempt for "normal politics" that characterized Weimar led to the immoderate politics of the thirties. National Socialism can only be understood in the context of the social and political conditions of Weimar, a peculiarly German blend of innovation and tradition. But at least Calleo avoids the current vogue of making lengthy excursions in search of the intellectual origins of Nazism or of dealing in analogies between Weimar and today's America. It may well be that the Weimar Republic came too early in German history, and that Germany is, as Hellmu Plessner once wrote, the *Verspaetete Natior* Only today has Germany finally completed it revolution of modernity and become a moder industrial society indistinguishable from it neighbors.

At the end of his book Calleo suggests that i is not only the Germans who should stud German history. But if the problem of moder industrial mass societies is to adapt old institu tions to new circumstances, then Calleo's gran style of history may be less relevant today. Th German problem in history was one of resolvin social conflict. Thus the vast output of Germa history, whether constitutional, intellectua diplomatic or political, needs to be harnessed t modern sociological and political science analysis.

The origin of the Holocaust, the World War guilt question, inflation and reparations, Bismarck's diplomacy, the *Ostpolitik*—these ar questions which in a moral and political wa have affected all of us in the twentieth century They are reviewed here provocatively from a broad geopolitical outlook. What has mad Germany so fascinating has been the impact o its politics not only on the personal destiny o every Central European but also on the fundamental questions of the social sciences.

CHARLES R. FOSTER

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Forecasting in International Relations: Theory Methods, Problems, Prospects. Edited b Nazli Choucri and Thomas W. Robinson (San Francisco: W. H. Freeman and Co. 1979. Pp. xii + 468. $29.95.)

This book gives a broad survey of forecastin in international relations in the 1970s. I discusses the philosophical problems of forecasting in detail, covers a wide range of forecasting methodologies, and deals with the problems of applying forecasting to policy makin in the United States and elsewhere in the world. However, the subtitle "Theory, Methods, Problems and Prospects" should be taken literall and as a caveat, as the articles discuss thes issues almost to the exclusion of "Results." Th book should be required reading for anyon interested in becoming involved in forecastin or using forecasts in international relations, and will be useful to those doing forecasts in othe policy areas, but contains little material o general interest.

Some form of forecasting is involved i almost all policy making but most forecast have been done on the basis of the training and intuition of the policy makers or their consultants. Since the record of history shows such forecasts to be anything but flawless, the methodologies of modern political science provide a plausible alternative. The ideal forecasting methodology would provide decision makers with a crystal ball which would foretell what is going to happen under any set of conditions. However, it is clear from the discussion in this book that the crystal ball is not forthcoming. Dozens of complications are noted by different authors; I would single out three as most important. First, there is the inherent unpredictability of politically important events such as deaths and natural disasters. Second, there is the complexity of social systems and the fact that the number of possible futures expands exponentially even in short time frames. Third, the policy community can react to a forecast crisis either to reduce the probability of the crisis (e.g., population growth) or at worst to exacerbate it (e.g., the "missile gap").

While forecasting uses many of the methodologies of the scientific study of social phenomena, it has had a difficult time finding a niche in that approach. As Cheryl Christensen notes, forecasts are part of but not all of the scientific approach. In general the policy community is uninterested in the theory underlying forecasts, and that neglect of theory is apparent in the book. Only one essay, by John Ruggie, deals in depth with international relations theory that might be used to construct forecasts. Generally, forecasting models appear to be constructed either of a series of plausible bivariate hypotheses, as in the systems dynamics approach, or else the theoretical approaches are implicit in the minds of the experts assembling the forecast. With few exceptions, the only theory one encounters here is probability theory; the lack of cumulativeness vis-à-vis the international relations literature is acute.

More bothersome than the lack of theory is the lack of *forecasts,* which a reader might reasonably expect in a book on the subject. Most of the examples used are ad hoc and loosely reasoned, the sort of illustrations one would present to an undergraduate class. These do nothing to engender confidence in the techniques. There are four notable exceptions: Thomas Milburn discussing the Michelson Project, Edward Azar discussing the COPDAB crisis warning model, Nazli Choucri and Marie Bousfeld discussing extensions of the Choucri and North *Nations in Conflict* (W. H. Freeman, 1975) model, and Robert Young and G. Robert Franco discussing long-range projections by C.A.C.I. All present relatively unambiguous forecasts with reasonably clear, though not replicable, descriptions as to how the forecasts were made. These, however, are only 4 chapters out of 28 and employ only a fraction of the methodologies suggested. The editors state in the preface that they are "painfully cognizant" of the absence of forecasts: so is the reader.

Despite the absence of actual forecasts, about a third of the book is devoted to the discussion of forecasting methodologies, including Delphi techniques, Bayesian analysis, econometric methods, systems dynamics, computer simulation, Markov chains and other techniques. Richard Ashley on Bayesian methods, Choucri on econometrics and systems dynamics (two chapters) and Azar on forecasting using events data are particularly good.

Throughout the book a healthy concern is shown for the normative issues in forecasting, a concern conspicuously absent from the earlier "systems analysis" forecasting concerning nuclear war, economic development and other social issues in the 1950s and 1960s. The essay by A. Michael Washburn and Thomas E. Jones is particularly good on this issue. The social science profession seems to have—finally—lost its innocence concerning the possibility of making value-free "scientific" statements in a value-laden policy world. In the policy world, unlike in formal logic, the truth of the consequent of an "If . . . then. . ." statement seems to imply the truth of the antecedent and in contrast to Milton Friedman's oft-cited suggestion, we probably cannot afford to make arbitrary assumptions solely on the grounds of accurate predictions.

But lest one becomes too engrossed with the possible applications of forecasting, Chester Cooper's essay on the uses of forecasting in Washington is a chilling reminder that even if one were to overcome the technical problems of accurate forecasting, that information would probably not be used. Accurate *current* information—as in the Bay of Pigs incident and during the Vietnam War—often fails to have an impact, so one could not expect any more of forecasts. Nevertheless, the Department of Defense has funded extensive research in crisis forecasting (see the March, 1977 *International Studies Quarterly* [Vol. 21]); and the Central Intelligence Agency has started to employ econometric forecasting. The State Department, on the other hand, appears to have little interest in developing new forecasting techniques.

In conclusion, forecasting in some form has been and will be an integral part of the policy process and is one obvious application of research methodologies developed in the social sciences. However, it does not appear that we are on the verge of any breakthroughs in the area, and forecasting may already be close to its limits of precision (which, I note, is itself a forecast). The problems presented far outstrip the methodologies and one is constantly brought back to the absence of examples, a silence which is unsettling. The book is presumably intended for internal communication within a small academic and policy community—and will doubtless serve that purpose well—but it seems to outline difficulties rather than solutions for political science generally.

PHILIP A. SCHRODT

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Tides Among Nations. By Karl W. Deutsch. (New York: Free Press, 1979. Pp. viii + 342. $17.95.)

Reviewing an anthology of Karl Deutsch's work is a far from simple assignment. The scope is incredibly broad, ranging from the social psychology of national loyalty to the rise and fall of empires. It is at once as detailed as the size of the industrial work force in 1955 and as sweeping in its generalizations as to suggest the basic and recurring patterns of nation building over the past five centuries. Further, his metaphors run the gamut from the thermostat to oceanic tides and currents. Despite this conceptual and empirical diversity, Deutsch's work shows an underlying unity, and this particular volume—whose articles date from 1940 to 1974—makes that unity abundantly clear.

That coherence lies, first, in Deutsch's focus on the relationship between the global system and the nations that constitute it. Second, there is the epistemological unity by which he skillfully combines carefully devised data sets with broad theoretical models. Third, and perhaps most salient to political science today, is the normative coherence, with description, explanation, prediction, and prescription side by side, carefully differentiated but explicitly intertwined. In a social science that is as much in flux today as it was when Deutsch's work first appeared, and as rife with division as when the behavioral revolution—of which he was a major instigator—first broke upon us, this is as valuable as his theoretical breadth and conceptual creativity.

Most of the chapters in *Tides Among Nations* revolve around an intriguing dilemma. That is, our nation-centric system has historically been a relatively war-prone one, yet there is little likelihood of restructuring the system quickly enough to make it much less war-prone in the nuclear-missile age. Put differently, nationalism has historically been a major concomitant of war, but while war must be reduced or eliminated in the near future—if we are not to suffer untold destruction—nationalism is likely to be with us for perhaps another century. This dilemma may, it seems, be broken down into one empirical generalization, one conditional prediction, and one unconditional prediction.

The generalization is that a system of sovereign territorial states is necessarily warlike, and this seems hardly controvertible. According to data from the Correlates of War project, we have experienced from three to thirteen international wars per decade, with a mean of six, since the Napoleonic wars, and there is nothing in the time plots to suggest either some clear periodicity or any secular trend, up or down. The conditional prediction is essentially an extrapolation of the above pattern: as long as the national or multinational territorial state is the dominant form of human organization, war will continue with deadly regularity. The unconditional prediction, however, is equally interesting, clearly central to the book's argument, but remains less than demonstrable at the moment: that nationality and nationalism *will* remain the dominant form of social organization in the global system for perhaps another century. Most of the articles in this anthology address that prediction, and they bring to bear an impressive amount of direct and indirect evidence, along with the rigorous theorizing and original speculation that so characterize the research and writing of this distinguished colleague.

In the first part of the volume, the evidence for the durability of the nationality preference is offered largely in terms of the lengthy historical process by which nations took shape. Not only does Deutsch find some impressive uniformities in that process; he also finds some radical transformations in a wide variety of social practices and institutions, many of which must be wholly or partly undone if we are to go beyond nationalism. More importantly, he finds that the material and psychic advantages have been greater than is often appreciated, suggesting that vested interests as well as habit stand as strong bulwarks against any immediate assault on the structural and cultural patterns associated with nationalism. Particularly noteworthy in this part of the book are Deutsch's creative use of quantitative indicators of social mobilization and his insightful analogies between coalition and competition in the economic and the thnic-linguistic spheres.

In part 2, we shift from an emphasis on the actors making for the growth and durability of ational consciousness to the problems of dentifying and measuring social distance and nteraction between nations. From the practices by which governments generate acquiescence, if not active support, of major groupings, to the elative propensity toward intra-national and xtra-national transactions, these four chapters nicely combine a range of important theoretical nsights with some ingenious indicators of nteraction, interdependence, and distance. Then in part 3, the focus is on the post-World War II processes and prospects of European integration. Drawing from the historical patterns discerned in the processes by which Switzerand, America, Italy, and Germany were integrated into relatively effective multinational tates, Deutsch examines the early development and subsequent slowing-down of the West European process. Also important here is the author's balancing of the factors—material as well as symbolic—that make for disintegration and secession, along with those that presage the formation of larger political units and security communities.

Then in the fourth and final part, Deutsch returns to the relationship between national ntegration and international conflict. While expressing the hope that "during the next half-century, world politics may become in some aspects more similar to domestic politics, and that some counterparts to intra-national welfare policies and income taxes may have to be found and accepted" (p. 8), the author's analyses in these last three chapters hardly offer grounds for optimism. To the extent that a certain level and distribution of wealth is a prerequisite for world peace, the data suggest that neither in growth nor in redistribution are we moving rapidly enough. Similarly, and making a point closer to his basic theme, Deutsch reminds us that nationalism is "an epistemological disaster" to the extent that "urgent and relevant messages from reality are overriden by unrealistic or irrelevant messages which this ideology [nationalism] prefers" (p. 302). Moving to other requirements for keeping the peace, or at least avoiding nuclear war, he notes two that seem dangerously distant: one is the acceptance by the wealthier nations of an international income tax, and the other is the renunciation of a nation's right "to initiate or escalate warfare without an international mandate" (p. 313).

In sum, *Tides Among Nations* offers a fascinating and provocative sampling of Karl Deutsch's impressive output since his arrival on the American academic scene. While he has been no more successful than the rest of us, here or abroad, in cracking the causes of war/conditions of peace problem, or coming up with a solution that is likely to be politically acceptable and effective, he has done more than almost any other scholar in moving us ahead on those two paths. His extraordinary capacity to bring historical facts, correlational knowledge, theoretical models, and quantitative techniques together should provide both a more solid base for the research of the decades ahead and demonstrate that creativity and precision, not to mention concern and competence, can and must inform the efforts of all serious social scientists. The likelihood of making the necessary global changes in time is not very good, but to the extent that we follow Deutsch's example, we nevertheless retain a fighting chance.

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Myth, Oil, and Politics: Introduction to the Political Economy of Petroleum. By Charles F. Doran. (New York: Free Press, 1977. Pp. x + 226. $12.95.)

Charles Doran's book suffers from ambivalence in purpose: its ostensible and real messages compete for attention.

To explore the role of myths in shaping energy policy, Doran devotes one chapter to each of the following myths: that oil prices are unfairly high; that Israel is the fundamental catalyst for OPEC unity; that profits of the oil industry are excessive; that divestiture of oil firms can solve the energy problem; that the International Energy Agency (IEA) reflects the solidarity of consumers; that OPEC is inherently cohesive. According to Doran, these myths serve important psychological and political functions for their adherents, such as creating certainty for individuals or enhancing loyalty to causes; but they contain substantial, albeit often subtle, elements of falsehood which go unrecognized by their believers. The persistence, influence, and irrationality of the myths, he suggests, have impaired U.S. energy policy.

Doran's observations seem to require—as he acknowledges—an analysis much like those produced for studying the role of symbols in politics. But, on this score, the book disappoints. Doran never satisfactorily explains why his list constitutes core myths. Moreover, he skirts the problem of finding tests for the impacts of myths on political processes and outcomes. For example, he observes that the U.S. public distrusts petroleum companies, but after offering a conventional explanation for this phenomenon, he does not demonstrate whether it affects the substance of policy, or merely its timing and form.

Ultimately, Doran's failure to explore the subtleties surrounding myths is not critical because the apparent theme of the book obscures its real focus; it is a series of essays disproving hypotheses which he considers erroneous. As such, the book is a modest, though uneven, warning against simplistic diagnoses and solutions of energy problems. It contains few materials which will be new to specialists. Its appropriate audience will consist of scholars and sophisticated political actors who have not specialized in the field.

The chapters on oil prices, the role of Israel, divestiture, and OPEC are the most rewarding. They set forth maxims drawn from welfare economics, theories of monopoly and oligopoly, and political science in a sensible and succinct manner. The essay on divestiture does an especially good job of showing the strategy's unintended consequences. It ignores, however, the best case for divestiture: one arguing that firms exceed the size necessary for optimum efficiency and, therefore, that divestiture (in the manner of action against Rockefeller's Standard Oil trust) should force disposal of subsidiaries to reduce size, not abolish vertical integration.

The chapter on profits in the oil industry correctly concludes that competition increased after 1945 and profits were not extraordinary. But its reasoning is shaky. It mistakenly looks at profits on refining operations as a good test of overall profitability. Refining profits were often low during years of very large profits on production because of the companies' oligopolistic strategies. More generally, it short-changes the evidence showing how corporate maneuvers slowed the decline of oligopoly and inadvertently eased the way to cartelization by OPEC.

The chapter on the IEA has the most claim to originality, but it is the weakest. It begins with a series of misleading emphases. For example, during the effort to install a minimum safeguard price for energy in the IEA there is little evidence, despite Doran's emphasis, that anyone worried much about the issue of windfall profits or thought the U.S. would act unilaterally. Doran also misses important aspects of the work done on creating national review programs, on monitoring the oil market, and on the design of arrangements for research and development cooperation which emphasized selective participation by members.

Doran suggests that most IEA countries are so different in their energy profiles from that of the U.S. (and, to a lesser extent, Japan and West Germany) as to make the organization ineffective. Unfortunately, the test of this provocative hypothesis is mediocre. The "Q-factor analysis" of elements affecting the "foreign energy dependence of consuming countries" (p. 130), the key indicator of difference, is inadequate. Some of the factors, like net capital inflows, have only slight connections to the level of dependence. Moreover, Doran is not adequately sensitive to the perennial problem resulting from giving the factors equal weight. In addition, the variables rely on data from 1970 and 1972, thus failing to capture such subsequent crucial swings as the ones for energy imports into the U.S. and the U.K. The ultimate predictions of coalitions capture neither key divisions over specific programs nor the general interplay of the U.K., Italy, Japan, France and Canada with the rest of the IEA.

Astonishingly enough for a book on myths, its hypothesis on the dynamics of the IEA also ignores the role of ideological factors, such as preferences concerning the regulation of markets. And it does not explore one major competing explanation for the IEA's difficulties: that the U.S.'s decline in power means that the IEA lacks a truly hegemonic power, which certainly must be singular in its profile, capable of organizing cooperative ventures. Thus, Doran's hypothesis, and the prescriptions drawn from it, are suspect.

In summary, for specialists this study lacks either the nuance or the originality necessary for a book to stand out in the flood of recent work. Nonetheless, Doran has synthesized a considerable range of valuable materials in a single volume which generalists may find rewarding.

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Presidents, Secretaries of State, and Crises in U.S. Foreign Relations: A Model and Predictive Analysis. By Lawrence S. Falkowski. (Boulder, Colo.: Westview Press, 1978. Pp. xii + 173. $16.50, cloth; $7.50, paper.)

Students and practitioners of foreign policy have long believed that the secret of successful foreign policy decision-making process. If the process "works," so the argument goes, the desired outcome will follow. Making the system work is often defined (among other ways) as increasing communication among bureaucratic participants, increasing the availability of information, encouraging the search for alternatives, and teaching policy makers to learn from their failures. These procedures are designed to maximize the rationality of the process and, by implication, to achieve the "best" outcome. Falkowski's effort to explain U.S. foreign policy behavior is of this genre.

Lawrence Falkowski develops a model of flexible foreign policy behavior, where "flexibility" characterizes a good or an effective foreign policy process. His model addresses three questions: Do U.S. leaders (presidents and secretaries of state) exhibit flexibility? Do different leaders exhibit different degrees of flexibility? What factors are related to a leader's flexibility or inflexibility?

Falkowski hypothesizes that differences in leaders' goal referents, goal themes, and policy themes (as reflected in their speeches, news conferences, and testimony before congressional committees) are related to differences in perception, and that variations in perceptions can be employed to explain the likelihood of flexible behavior on the part of any decision maker. Specifically, the greater the congruence between referents and the current situation, the more flexible is foreign policy behavior. And the greater the congruence between referents and goal themes in terms of a particular issue, the more flexible is foreign policy behavior. (The implicit hypothesis, of course, is that the greater the flexibility in foreign policy behavior, the "better" is the policy.)

Falkowski tests these and other hypotheses by focusing on those situations most likely to encourage flexible foreign policy behavior, namely, crises that have been deemed failures. If leaders exhibit any flexible behavior, so Falkowski hypothesizes, they should do so after they have failed in a crisis. On the basis of his analysis of the pre- and post-crisis perceptions and behaviors of presidents or secretaries of state from Truman to Nixon, Falkowski concludes that his hypotheses are supported. That is, the extent of foreign referents and the degree of congruence between referents and goal themes explain flexibility.

On the positive side, this volume demonstrates that psychological variables can be addressed more or less successfully by non-psychologists, and that elements of memory can be identified, defined, and operationalized.

On the other hand, Falkowski's model of flexible foreign policy raises questions in my mind which are not adequately addressed in this volume. First, the model is not really a model of foreign policy *behavior*, but a model of the flexible *leader*, defined in terms of the leader's dispositions. The independent and dependent variables in the research design are "themes"–the "intellectual baggage" of foreign policy decision makers pre- and post-crisis (p. 58). And yet Falkowski defines flexibility earlier in the volume as "a change in observed behavior from a previous pattern of behavior" (p. 4). In short, there appears to be some confusion surrounding what the model is designed to explain.

A second question concerns Falkowski's decision to examine only crises that have been deemed failures. A valid test of the model requires examination of three other logically possible situations: crisis successes, non-crisis failures, and non-crisis successes. In the absence of data concerning perceptions and behaviors in these situations, conclusions derived from crisis failures are less than completely persuasive.

A third question is one that arises not only from a reading of Falkowski's study, but also from analysis of much of the "successful process" literature. Falkowski assumes that flexibility results in favorable foreign policy outcomes. But flexibility could also be characterized as inconsistency or unpredictability. Flexibility could suggest the absence of "policy" (at the behavioral level) or the presence of instability (at the intellectual baggage level). For instance, President Carter's "flexibility" on the enhanced radiation warhead issue brought charges of inconsistency from both domestic and foreign critics. The assumed linkage between successful process (particularly if defined as flexibility) and desired outcome is tenuous at best.

In summary, Falkowski's model of flexible foreign policy raises many hard research questions which remain to be addressed.

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The New Politics of Human Rights. By James Avery Joyce. (New York: St. Martin's Press, 1978. Pp. xi + 305. $19.95.)

Victims of Politics: The State of Human Rights. By Kurt Glaser and Stefan T. Possony. (New York: Columbia University Press, 1979. Pp. xxiv + 614. $35.00.)

The reading of any national newspaper on any given day in this country is bound to turn up at least one item identifying a situation exhibiting the lack of effective implementation of international human rights principles. The conditions of human life are affected by an unending series of man-made catastrophes. Nevertheless, today, more than at any other time in the history of mankind, there is a rising tide of hope for the protection of the individual's human rights against the state.

James Avery Joyce, in the preface, acknowledges that *The New Politics of Human Rights* is "but a broad survey aimed at the cultured reader." It is, also, "not a book for lawyers"; hence, "a legalistic style has been avoided as far as is compatible with accuracy and clarity." Furthermore, the legalistic approach is abandoned for the reason that a "serious study of human rights has become today an interdisciplinary exercise, involving history (origins), geography (frontiers), diplomacy (negotiations), sociology (group structure), psychology (motivation), ethics (standards), and many other formal disciplines, as well as constitutional and international law." The "cultured reader" will find an absence of any interdisciplinary approach, and although the title suggests a survey from a political perspective, it too is woefully lacking. In sum, the book has few pretensions and even fewer achievements.

Joyce gives the reader a synoptic view of the 1948 Universal Declaration of Human Rights, the 1949 Geneva Red Cross Conventions, and the 1977 changes in the Laws of Armed Conflict, without analysis of the political challenges leading to their final adoption. His survey of past human rights accomplishments involves slavery, humanitarian intervention, minorities, labor standards and the mandatory system. Recent examples of states being caught *in flagrante delicto* include situations in South Africa, Chile, Israel and the Soviet Union, among others. Not without merit is Joyce's treatment of the doctrine of self-determination by the third world, who are now the advocates of neo-colonialism. Another strength of the book lies in Joyce's assessment of scientific and technological advances with their concomitant encroachment and constraint upon the rights of privacy and death. On the other hand, the topics of genetic engineering and organ transplants receive only cursory inspection on an elemental level. While the examination of how the 1966 International Covenants and the Council of Europe's European Convention operate is adequate for the lay person, a political perspective would have enhanced its value. The need for a UN High Commissioner for Refugees and to a lesser extent, a British Bill of Rights, are familiar nuggets assayed without new insight. Finally, Joyce advocates that the purpose of the new legal order will be the individual.

In *Victims of Politics* Kurt Glaser and Stefan Possony have produced a definitive work with a catalog of human suffering encompassing the spectrum of human rights violations. There is an enormous amount of scholarship, sifting and refining of materials, deep thought and erudition in this volume. Where does this panorama of pain and suffering lead to? Who is responsible? The authors provide a number of sound explanations for the limitations on the implementation of human rights, explanations of human perversity and the role of ideology, and they point out problems inherent in the practical application of human rights standards.

Indifference to injustice is not a modern-day phenomenon. The authors show that "we need to revise our political theory so that it will place people first and abstract principles in a lower priority" (p. 544). They do not have the recipe for a panacea to cure this cancer permeating our planet. However, the empirical, psychological theoretical phenomena which lead to violations are amply reviewed.

The recognition and implementation of human rights principles is a universally valid moral command. Glaser and Possony agree that to succeed, a human rights policy must not be an instrument of publicity coups, nor have double or multiple standards. This policy should only focus on consistent violations rather than occasional lapses; it must be low-key, a polite exercise in persuasion, not aimed to arouse hatred or aggression. Any successful policy will contain its own guarantee of due process.

Despite the mass of facts illustrating abuses of human rights, Glazer and Possony feel that "the road toward greater realization of human rights remains open. It is long and strenuous but progress can be made step by step" (p. 555).

This book stimulates the reader. Not only have the authors whetted the appetite of those who want to learn about human rights violations and its causes, but they have provided a smorgasbord on the subject. Those seeking to dine elsewhere will find a helpful 30-page bibliography.

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Crisis Resolution: Presidential Decision Making in the Mayaguez and Korean Confrontations. By Richard G. Head, Frisco W. Short, and Robert C. McFarlane. (Boulder, Colo.: Westview Press, 1979. Pp. xxv + 323. $20.00.)

In May 1975, Cambodian armed forces

seized an American cargo vessel and its 40-man crew. In August 1976, North Korean forces killed two U.S. officers engaged in a tree-trimming detail in the tense north-south border area. How the Ford administration handled these "low-intensity crises" (p. xix) receives detailed scrutiny from three U.S. military officers associated with the National Defense University. Drawing on extensive inside information—including interviews with former President Ford and Secretary of State Kissinger—the authors show how the U.S. response was affected by such factors as "key policy makers' belief systems and the unique situational context" (p. 243), and the limits of available information.

The Mayaguez seizure came two weeks after Saigon's collapse, and Ford and Kissinger were determined to respond strongly, though U.S. forces in the area were limited, and the crew could not be precisely located. So after two days of military preparations and unanswered diplomatic messages, U.S. Marines launched a rescue effort. They met heavy Cambodian fire and 15 of them died; meanwhile, unknown to them and Washington, other Cambodian forces were releasing the crew. In the Korea case, U.S. armed forces asserted their rights by cutting down the tree where the incident had occurred. A limited apology from North Korea led to negotiations dividing the border area into north and south portions, reducing the likelihood of future incidents.

Going beyond crisis reconstruction, Head, Short, and McFarlane address broader theoretical and practical concerns. For academics, there is a cogent review of the literature on foreign policy crises, development of a conceptual framework, background on the National Security Council and Worldwide Military Command and Control systems, and a number of insightful conclusions. Head and his colleagues find that these crises gave the president particular opportunities to control policy, that bureaucratic interests were subordinated, that the formal crisis management system was used, not circumvented, and that, despite officials' sustained engagement, there was no evidence of "abnormal stress" distorting their behavior (Ch. 7). The book also provides intelligent suggestions on crisis management.

The authors' basic sympathy toward the participants, processes, and outcomes sometimes colors their conclusions. President Ford is praised for "removing politics entirely from the discussion of virtually every major issue which came before him" (p. 72). His 1976 refusal to cancel Kissinger's Rhodesia mission is the example cited; ignored are SALT and Panama, on which Ford pulled back that same year. The Mayaguez action is labeled a "successful U.S. military operation" demonstrating "a strategic resolve worthy of a great power" (p. 148). The GAO criticism that the crew's release was unrelated to the Marine assault is quickly dismissed because this release "was not known" *to U.S. officials at the time* (p. 145), the U.S. had to show it "would act with firmness to protect its interests" (p. 110), and the worst-case outcome—the Cambodians' hiding the crew and humiliating the United States—was avoided. These are important considerations, and the authors are right to underscore the uncertainty within which officials worked. In turn, less certainty about the rightness of the course chosen would seem appropriate.

Head, Short and McFarlane also take formal procedures at face value, sometimes without exploring whether they were actually followed. In one background chapter, they insist repeatedly that Nixon and Kissinger operated an internally open policy process, citing as evidence mainly the system's formal description and design. Looking specifically at the Mayaguez incident, the authors begin by noting that *normal NSC procedure included "attempt[ing]* to determine the motives of the other party and its objectives" (p. 109). But the next 40 pages almost totally ignore this matter. Was the seizure the act of a local commander or Phnom Penh? The answer might have made a difference in how the U.S. should respond, and if this question was indeed neglected, this would seem a serious process flaw. If so, it goes unilluminated.

But despite such limitations, this book is an important addition to the literature on specific foreign policy crises. Although skewed by a sympathy that inside analyses often show, it gives us more than adequate compensation in the inside information the authors provide, and in their practical wisdom about how government works.

I. M. DESTLER

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Quantitative Approaches to Political Intelligence: The CIA Experience. Edited by Richards J. Heuer, Jr. (Boulder, Colo.: Westview Press, 1978. Pp. x + 181. $16.50.)

The message of this book is that the social science research practiced by the overwhelming majority of quantitatively oriented scholars is a waste of time—at least by certain standards of judgment. The editor, Richards J. Heuer, Jr., and the several authors are much too circumspect to make any such explicit charge against their fellow citizens. However, in the most polite ways possible they point out that the available quantitative research was of little use when they began to try to upgrade their own forecasting and other analytical techniques.

The many reasons for the inappropriateness of the bulk of social science research are well described by explicit statement and example in the book. Some of the most damning comments are made almost in passing, as, for example, when Heuer in his introduction mentions that "the quantitatively oriented scholar can easily limit his work to those variables that can be operationalized, but the government analyst seldom enjoys that luxury" (p. 4). The implicit charge here is that quantitatively oriented scholars do, in fact, pick their research topics on the basis of those things that can most easily be measured.

The book also goes on to talk about why the models and methods used by scholars are inadequate for government use. There is no doubt that many of us will be happy that our work is so irrelevant to practical purposes, especially those practical for the CIA. For those people the book will be quite conforting. At least it will be, as long as they do not raise the question of whether the work they do interests *anyone* except another scholar.

Perhaps the most radical theme of this book (again, only implicit) is the major use of expert-generated data as a source of information on which to perform various statistical and modeling techniques. Three of the seven studies rely almost exclusively on expert-generated data. Even more interestingly, the use is not the conventional Delphi approach of eliciting forecasts of future conditions. There is much more obtaining of specific estimates of *current* conditions or the relationship between current conditions and future events. The results here are mixed. In particular some of the studies have tremendously complex and elaborate methodologies that probably do more harm than good to the data collected. One such example is the attempt by Harold E. Dahlgren to operationalize a theoretical model that will forecast domestic violence. Since I had a chance to evaluate this study before it appeared in the book, I am aware (even more than Dahlgren) that the data collected had a high degree of reliability (consensus among the respondents) and at least "face validity" (variations in the predictors did covary with incidence of violence in the countries studied). However, the elaborate model that Dahlgren presents sometimes tended to obscure this quite strong performance of expert-generated data.

The other use of expert-generated data includes formulating Bayesian analysis estimates for Middle-East conflict and making forecasts about future events in Rhodesia.

These studies, along with those that use more conventional techniques such as regression analysis, content analysis, events data, and scaling of UN voting behavior, show the great potential of quantitative techniques. It is unfortunate that much of government work, as indicated by this book, has to begin almost from scratch with so little help from previous work by academics.

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Dimensions of China's Foreign Relations. Edited by Chün-tu Hsüeh. (New York: Praeger Publishers, 1977. Pp. xiv + 293. $20.00.)

This volume, edited by C. T. Hsueh, is a collection of ten articles which treat different aspects of contemporary Chinese foreign relations, but among which there is no common perspective or clear division of labor. The authors, most of whom are specialists on Chinese politics, do not ask similar questions, nor have they structured their analyses so as to complement the other essays in the volume. Davis Bobrow opens the collection with an introduction suggesting an analytical matrix in terms of which the articles might somehow be related to each other, but in spite of this valiant effort, Bobrow himself concludes by emphasizing questions which the authors might have asked rather than those actually posed.

Lacking a coherent organizing structure, the book can best be read, it seems to me, for the contributions made by the separate articles. The topics include: analyses of China's involvement with the two superpowers (by Steven I. Levine, Chün-tu Hsüeh and Robert C. North, and King C. Chen); studies of Chinese relations with Japan, Korea, the Palestinians, and Latin America (by A. M. Halpern, Chün-tu Hsüeh, Yitzhak Shichor, and Robert L. Worden, respectively); and, finally, an investigation of Peking's policies toward the Overseas Chinese (by Shao-chuan Leng) and a case study of the impact of the Cultural Revolution on Sino-Nepalese relations (by Roger L. Dial).

In my opinion, the articles are as uneven in quality as they are diverse in subject matter. Let me briefly comment on four of those tributions which I found to be among the re interesting.

The first, Steven Levine's "Soviet-American valry in Manchuria and the Cold War," amines the relationships among Moscow, shington, Chiang Kai-shek's Kuomintang vernment, and the Chinese Communist party ring the final stage of China's civil war 945–1949) following the end of World War

While the United States and the Soviet ion seemed to share the common objectives encouraging the formation of a coalition vernment comprised of the two hostile Chi-se political movements (the KMT and the P) and of generally refraining from direct litary intervention in the Chinese civil con-t themselves, both Mao Tse-tung and Chiang i-shek sought cooperation with each of the o foreign powers—or, at least, attempted to n the support of one and to neutralize the her. Levine disputes the popular notion that ferent American policies toward the CCP at is critical juncture might have prevented the bsequent 20 years of Sino-American hostility 950–1970), but he does show the tremen-us influence of the change of American rsonnel in China on how events there were ing reported to Washington, i.e., after Ambas-dor Hurley replaced officials advocating U.S. operation with the CCP (like John Service d John Paton Davies) with those who saw inese communism from more of a cold war rspective.

C. T. Hsüeh, surveying Chinese policy ward Korea over the entire period since 1949, alyzes the Sino-Korean relationship in terms the historical links between the two coun-es, the principles of proletarian international-n, and the realities of political power in the gion. Hsüeh seems to suggest that in spite of ina's verbal support for the reunification of e Korean peninsula and the withdrawal of merican troops (as President Carter has pro-ised), Peking might actually be willing to cept the status quo as the best of the likely ternatives. In fact, since the political outcome any effort to unify Korea is indeed unpre-ctable, one wonders if all of the major powers volved in the area (the U.S., China, Japan, d the Soviet Union) might not prefer the atus quo of a divided Korea—for China, pecially since Peking's recent unhappy experi-ce with a reunified Vietnam.

A third chapter of particular interest is "The lestinians and China's Foreign Policy" by itzhak Shichor. Arguing that China's policy ward Israel and the Palestinians has been sentially a reflection of Peking's global policy, ichor traces Chinese relations with the Palestinians through three periods: the early years of China's alliance with the Soviet Union against the United States, the period from the mid-1960s when Peking confronted both superpowers as enemies, and, finally, beginning in 1971, the years of growing cooperation with the United States and continued confrontation with the Soviet Union. His point is that policy toward Israel and the Palestinians was shaped by the different requirements of China's global strategy during each period. Shichor makes good use of a hitherto virtually untouched goldmine of materials on Chinese foreign relations, the *Shijie zhishi nianjian* (World Knowledge Yearbook) and *Shijie zhishi shouce* (World Knowledge Handbook) series; and he has produced an impressively detailed and balanced analysis—in short, a valuable study of a topic which has received little previous scholarly attention.

The final article in this collection is Roger Dial's study of the Chinese foreign affairs bureaucracy which is conceived in terms of a general notion of organizational response to a crisis situation. Dial treats the domestic chaos of the Great Proletarian Cultural Revolution as a challenge to the established bureaucratic patterns of Chinese diplomacy, and he investigates the case of Sino-Nepalese relations, focusing on activities undertaken by Chinese Foreign Ministry professionals to protect their organization from Red Guard efforts to disrupt their normal organizational procedures. The result is a thoughtful case study which suggests lines of analysis which might fruitfully be pursued in subsequent studies of the role of the Foreign Ministry bureaucracy in Chinese foreign relations.

PETER VAN NESS

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The General Assembly of the United Nations: A Quantitative Analysis of Conflict, Inequality, and Relevance. By Kurt Jacobsen. (New York: Columbia University Press, 1978. Pp. 209. $15.00, paper.)

The book is based on research done at the International Peace Research Institute (Oslo), involving approximately ten years of quantitative analysis. The purpose is to "analyze some of the functions of the United Nations during the time the organization has existed" (p. 14). The book opens by treating the United Nations in terms of Almond's and Powell's framework to analyze political systems that include the categories of "capabilities," "conversion processes" and "system maintenance and adaptation." From this point of departure, Kurt Jacobsen attempts to index "capabilities" through the scale of assessment for regular budget, "representation" through numbers serving in appointed and elected positions in the United Nations, "behavior" through presentations of proposals, participation in debates and voting on proposals, and "voting and interaction" through estimates of agreement in respect to voting behavior and as measured by co-sponsorship of proposals. Having established how he is going to index various key variables, Jacobsen then proceeds to evaluate the interest articulation and aggregation function of the United Nations in various ways. In this respect, "one's conclusion would have to be that there has been a shift-over time from the conflict model towards the consensus model" (p. 67).

Does this mean, then, that the world itself is becoming more peaceful? Jacobsen thinks not. "Our main point, however, is that one cannot and should not regard the U.N.'s relative "peace system' separately from that of the 'peace' we are trying to create in the outside world. The observed levels of consensus and conflict would have to be evaluated in light of the levels of consensus and conflict that reveal themselves in all parts of the world, not just in the corridors in New York" (p. 67). In evaluating system maintenance, adaptation and the development of legitimacy, Jacobson finds that the United Nations has become less dominated by the East-West conflict, that the group of nonaligned nations has expanded considerably, and that, in general, the United Nations has become more "balanced." "The end result is, however, that the organization is less unbalanced in a Southern direction than it was in a Western direction in earlier periods. The extreme isolation of the minority group–the East–has been broken" (p. 127). In evaluating equality versus power, Jacobsen finds that although inequalities exist at the UN, reflecting power, they are less apparent in the UN system than in the non-UN world and that the explanation, at least in part, can be found in the one-nation, one-vote rule.

On the question of relevance, Jacobsen feels that the United Nations has become more "unrealistic" since the resolutions do not reflect, as much as they have in the past, the interests of the stronger, more developed states. He argues: "the products of activities become paper resolutions with limited consequences for the situation in the 'real' world" (p. 170). This, however, is admittedly a "Western" position, and from the viewpoint of the less-developed states "no action at all may be preferred to action according to the wish and will of t powerful nations" (p. 170). Regarding t impact of socialization and communicatic Jacobsen stresses the broadening of perspectiv that are likely to develop in delegates throu their interactions with others holding diverge viewpoints. That is, he feels that the Unit Nations may raise levels of mutual predictabi ty which, in turn, may reduce levels of tensi and conflict.

In general, the book is highly recommend to serious scholars of the United Nations. T greatest weakness is Jacobsen's tendency ignore numerous significant contributions co cerning the United Nations by Rummel, Ru sett, Wittkopf and others, many of which a not even listed as references, that are high relevant to his effort. In spite of this weakne however, Jacobsen has produced a high-qualit innovative and readable contribution to t literature on the United Nations.

JACK E. VINCENT

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U.S. Foreign Policy in Asia: An Appraisal c America's Role in Asia. Edited by Yun hwan Jo. (Santa Barbara, Calif.: ABC-CLI Press, 1978. Pp. vii + 488. $19.75.)

After the communist victory in Vietnan America's commitment to its containment pol cy in Asia was subjected to anxious evaluatio Indicators were clear that America would wit draw from its forward lines on the mainland t its Pacific island bases. By 1976, this withdraw al seemed to bode ill especially for Sout Korea, a country that seemed fated to repea South Vietnam's mistakes. In 1976–197 therefore, experts were invited to three sym posia hosted by the Center for Asian Studies, a Arizona State University, to reassess America' role in Asia and to examine the Korea question in relation to Vietnam. More than 3 experts on these and on other involved coun tries participated. This volume is a result.

Like other compilations of conference pap ers, this one lacks coherence. The authors of it 19 essays and contributors in six discussion communicate out of different backgrounds an values and discuss different subjects. The include critics and defenders of the disparat policies of the Soviet Union, North Korea South Korea and America. Several schola make important recommendations for new poli cies.

For their research materials, these expert relied mainly on their readings of newspapers periodicals, books and official reports. Since many of them are area specialists, they also drew on their intimate knowledge of relevant countries. In the main, they omitted explicit theorizing, quantitative analyses, or references to global political considerations.

Despite their differences, the experts shared the views that American power in and commitment to the defense of Asia has been reduced by Vietnam's victory, that the big four powers in Asia—U.S., USSR, China and Japan—did not seek any radically destabilizing changes in Korea, that the stability of Northeast Asia would not be secured until the two Koreas were neutralized and/or reunified, and that Vietnam, in 1976, was primarily concerned with its own economic development.

Part 1 examines the reasons for America's intervention in Vietnam and Korea. Soon S. Cho surveys familiar history and concludes that America's initial division of these countries was caused more by the ignorance, imprudence and pragmatism of U.S. policy makers anxious to enlist the support of Russian or French colonialists than by any rational plan. Robert Oliver, in the style of a personal memoir, recalls his experience as advisor to Syngman Rhee and echoes many of Cho's criticisms. Gareth Porter represents the early policies of America in Vietnam (1954–1963) as based on the vain hope that they could counter the forces of popular revolution where the French had failed. Selecting data from official reports and personal interviews, Porter depicts American policy makers as consciously designing an economic aid policy that would favor the rich. While his thesis is well argued, it plays down consideration of other relevant factors such as the softness of Saigon's bureaucracy, the stubbornness of Diem, and the Americans' lack of knowledge and control of the countryside.

Douglas Pike's succinct piece on U.S. policy toward Vietnam in 1976 reads like the outline of a government position paper; it depicts U.S. policy makers as waiting "to let the dust settle" while, hindsight suggests, the Russians "made hay."

Jane Werner bases her impressions of Hanoi's foreign policy on extracts from official statements; she hazards interesting speculations on Hanoi's future policies with mixed results. Scott McNall's essay on the U.S. rationale for a limited war in Vietnam proposes that a specific policy of limited conventional offense was aimed at demonstrating the credibility of a general policy of unlimited nuclear defense. While he argues persuasively, the rationales for U.S. intervention appear more plural than singular and varied according to time and circumstance.

Phan Thien Chau appeals for a more accommodating U.S. policy toward Hanoi. Donald Weatherbee evaluates the impact of communist Indochina on Southeast Asia's security and describes new conditions defining a new environment affecting the foreign policies of regional actors at three levels. Weatherbee's analytic construct is unusually illuminating; he generates new insights from old data.

Part 2 examines American policy toward Korea. Gregory Henderson describes the negative effects of current repressive and militaristic policies in Korea and recommends that the four big powers cooperate to restrain their Korean clients and proceed toward stabilizing policies. In an emotional essay reflecting similar concerns, T. C. Rhee castigates South Korean policies that threaten to erode popular support by increasing domestic misery and foreign dependence. Anthony Khang rejects U.S. Asian policy and appeals for Korean unification on the communist model. Fred Carrier faithfully advocates official North Korean policies. Pyong Choon Hahm makes a contrary case for the South Korean government.

Part 3 analyzes America's foreign policies in relation to other Asian countries. Bernd Kaufman, an East German expert, ably presents the Soviet view of U.S. Asian policy. Yung H. Park's excellent article on Japan's policies toward Korea and America is one of the least prescriptive in the volume. Tracing the changes of Japanese policy toward the two Koreas in the 1970s, he perceives them as resulting from the changing pressures of domestic political party factions and business groups.

Douglas Mendel's spirited advocacy of a pro-Taiwan policy is apparently derived from his stout loyalty to that beleaguered government and from his reading of sympathetic sources. Robert Youngblood's essay on Philippine-American relations is gracefully written, coolly presented and solidly documented. Tracing the efforts of various nationalistic governments to develop independent foreign policies, he focuses on the Marcos regime and presents evidence indicating that, despite contrary claims, the New Order's economy and security remain dependent.

Robert Scalapino's paper on U.S. policy toward South Korea is a review of recent developments which plays down the problem of human rights, cheers signs of economic progress and carefully evaluates the interests of the big powers in Northeast Asia. In light of these factors and a promised reduction of U.S. forces, he prescribes a strategy for Korean demilitarization and negotiation that involves all these participants in a plan for reunification.

Although several of these essays are seriously dated because of subsequent interstate hostilities, by new efforts at negotiations and by U.S. recognition of Peking, this volume contains so much pertinent information and so many insights on the subjects at issue that it should prove most useful to interested graduate students, to area specialists and to policy makers.

JOHN JAMES MACDOUGALL

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The Leninist Response to National Dependency. By Kenneth Jowitt. (Berkeley: University of California Institute of International Studies, Research Series No. 37, 1978. Pp. ix + 85. $2.50, paper.)

The recent vogue of dependency theory, derived mainly from Latin American studies, cries out for a searching critique by scholars familiar with the origins and historical evolution of Leninism in Eastern Europe. This booklet by Kenneth Jowitt constitutes, one may hope, a beginning of that critique. Jowitt's purpose is to examine Leninism—the doctrine and the movement—as a response to political underdevelopment. Apart from liberal individualism, Jowitt considers Leninism to be the principal response. Like dependency theorists, he accepts the generalization that the critical aspect of political underdevelopment has been dependency upon the more advanced societies of Western Europe and their overseas progeny. His task, therefore, is to show how and why Leninism constituted a way of overcoming dependency at the international level, or, to be more exact, for replacing dependency on the Western European system by dependency on the Soviet Russian system. The new pattern, which he labels "combined substitution," is a transformation of countries' "domestic social order and international position *under the deliberate organizational auspices of a great power with whom they shared an ideological affinity*" (p. 79).

To show how Leninism came to constitute an alternative, Jowitt must, of course, refer to development theory. Drawing on his intimate knowledge of Romanian development, he substantiates Frantz Fanon's argument (anticipated by Romanian thinkers) that avoidance of a dependency situation requires the development of a national culture. The *modern* international order tends to replicate the *traditional* peasant social order as a strongly hierarchical status system, with "an international 'big man' patron who is expected to allow national 'small boys' to exercise control in their own bailiwicks in return for diffuse services to the patrons" (p. 26). Hence, for local elites, the international relationship, though a source of resentment, is familiar, therefore basically acceptable. Consequently, the elites adopt premature liberal institutions and styles to make themselves intelligible to Western patrons, until and unless a fully independent national cultural consciousness arises. This interpretation appears very plausible for some international relationships and perhaps still more for internal ethnic relationships of dependency. Jowitt's interpretation is also a welcome corrective to the stress on economic and political factors which has commonly pervaded dependency theory. In fact, I can only regard Jowitt's analysis as "a sociopsychological interpretation of . . . dependency behavior," although (p. 29) he categorically rejects this characterization.

Possibly recognition of the socio-psychological nature of his interpretation would have compelled Jowitt to recognize that his fundamental assumptions (though not his insightful treatment of Leninism) are much like Parsonian development theories, also ultimately derived from Weber. Instead, Jowitt (like all too many Eastern European specialists who turn to general theory) employs terms and concepts in idiosyncratic ways. He contrasts "status societies" with "class societies" without recognizing that, since Weber, generations of sociologists have accepted "status" and "class" as pervasive social characteristics. Nor is Jowitt's use of "impersonal" versus "trust" relationships an advance over the generally accepted "universalistic:particularistic" dichotomy. More seriously misleading is his attempt (p. 14) to develop a conflict model of changing family structures. Jowitt's belief that the nuclear family is a new phenomenon is simply wrong; it was widespread in Western Europe by the fourteenth century and may—rather than the "Protestant spirit"—constitute the key factor in the emergency of individualism there. To assert as Jowitt does (p. 14) that such a slowly evolving social form exists in a "kto-kovo" relationships with the extended family is a misuse of both the conflict concept and Lenin's polemically sharp adaptation of it.

Conversely, Jowitt is right in ascribing fundamental significance to Leninist transformation of traditional peasant extended family relationships, where such relationships are salient. However, he does not give sufficient attention to the crucial distinction *for peasants* of the *manner* in which Leninist regimes attained power. In my view, characteristics such as provision of heroic models and substitution party loyalty for traditional village solidarity e incomparably more significant in Commu- ist-led guerrilla struggles, from which Jowitt eferring to Ho Chi Minh, Malaysia, etc.) erives most of his examples. It may be that in omania comparable substitution patterns have een imposed successfully from above, and onsequently contribute to a modernized social rder. It is incontestable that the *initial* reac- on of Romanian peasants (before massive oviet force *compelled* disruption of the old llage patterns) was rejection of *artificially* troduced guerrilla solidarity models. The arti- ciality of Leninist substitution patterns in ther Eastern European countrysides (apart om Yugoslavia), in contrast to the consider- ble success of such patterns among mod- rnized proletarian elements, appears to be even ore evident, especially in Czechoslovakia and e German Democratic Republic. In other ords, though Jowitt presents a seminal inter- retation of the relationship of certain types of eninist regimes to peasant transformation, his terpretation should not obscure the funda- ental *Soviet* model of transforming the hos- le, or at least apathetic village, by cadres based n urban fortresses.

JOHN A. ARMSTRONG

niversity of Wisconsin, Madison

areer of Empire: America and Imperial Expansion over Land and Sea. By George Liska. (Baltimore and London: Johns Hopkins University Press, 1978. Pp. xi + 360. $17.95.)

Career of Empire is advertised as a com- anion volume to *Quest for Equilibrium,* eorge Liska's earlier effort to interpret Ameri- an foreign policy in terms of equilibrium, or alance-of-power strategy. *Career of Empire* oncentrates on a second motif—the impulse oward hegemony—and uses general proposi- ions about the creation, consolidation, and issolution of empires drawn from Roman and ritish history to examine America's imperial xperience.

A reviewer of the first study wrote that it ould repay "those who have the time and tamina to wrestle with close reasoning, com- lex syntax, and a high level of abstraction." he same can be said, if less euphemistically, bout this essay. It is a maddening book—a rovocative book written in prose frequently so urgid that the mind boggles. One typical xtract will illustrate the need for "time and tamina." In discussing the consequences of elegating defense responsibilities to client- tates, Liska writes: "It [i.e., the range of the imperial power's controls] would then comprise only activities and factors apt to inhibit seriously the crystallization and continuous operation of near-autonomous regional balance-of-power systems that would have taken the place, in the devolutionary context, of a partially regionalized enactment of the global or central balance attendant on mere delegation" (pp. 278–79). My objection to verbal contortionism of this sort is not just aesthetic. Language should be a tool to clarify and illuminate, not mystify and obfuscate. In this particular case, the substance is difficult enough.

There are three essential points to Liska's argument: (1) American policy has followed an imperial course comparable, *mutatis mutandis,* to that of Rome and Britain; (2) the American Empire, however, was not acknowledged until that moment when it was explicitly rejected (erroneously, in Liska's view) on both moral and utilitarian grounds; and (3) the dissolution of the American Empire was owing primarily to internal factors. It was an act of self-abdication, neither heroic (like the Roman) nor honorable (like the British), but an act which, precisely because it was voluntary, may be reversible.

Liska believes that such a reversal—a reexpansion of the American Empire "in scope or institutional depth" (p. 336)—could (and should) result from central (Soviet) threats to American or Allied security, regional hegemonism by [Vietnam?] or fragmentation of [Iran?] middle-range powers, economic aggression by natural-resource monopolists [the Arabs?], or a renovation of the domestic political elite.

Superficially, at least, all this appears to be merely an erudite restatement of recent demands that America cast off its Vietnam-Watergate-induced posture of self-effacement and resume a vigorous pursuit of its national values and interests. And if it were only that general principle at issue, then Liska's exhortation would be unobjectionable. After all, the dangers and challenges are real.

Career of Empire, however, is intended to be much more than a simple protest against American passivity or impotence in international affairs. It is intended to demonstrate that the protection of American values and interests necessarily entailed *imperial* expansion, since "the flaws [of empire] were organically of a piece with the accomplishments" (p. 334), and that only by arresting the internal rot that has sapped its will-power [will-to-power?] can America reconstitute its empire and thus defend those values and interests.

The argument is complex, tortuous, and afflicted by an inordinate fondness for alliterative, tripartite classification schemes. Liska begins by identifying the three uniform bases or sources of expansion (security, sustenance, system-maintenance) and then describes the three phases of empire building (predation, preclusion, terminal expansion), each with its typical relationship to economic factors (p. 53). From the Roman and British experience, he also abstracts three phases of ruling-class transmutation (trusteeship, transactional control, trauma), the last of which is associated with imperial decline, caused by three general types of challenges (p. 92) and the loss, inevitably, of the three specific props of empire (p. 97).

The framework is inspired categorization, but it is so all-inclusive and indeterminate (internal *and* external causes, security *and* material motivations) that it adds little, in understanding the rise and fall of empires, to the explanatory or predictive power of such banal variables as capacity or a permissive international system. Its elasticity is obviously amenable to Liska's main thesis, but even with much stretching and bending it is hard put to accommodate the American eagle. For, as Liska himself is forced to concede, this empire is a very rare bird. It was hatched without the typical nurturing warmth of a security or economic crisis (pp. 109–12) and was reared by no typical parents, in the form of an imperial elite or ruling class (p. 169). It developed no typically formalized, centralized structure (p. 269) and was sustained by no typically imperial ethos or romanticism (p. 306).

Small wonder, then, that only the "domestic counterelites" were easily able to recognize this empire. After all, like Liska himself, they understood things in "systemic" and "structural" terms. In particular, they saw an American Empire—in Vietnam and elsewhere—as the ineluctable projection of monopoly capitalism at home. But who were these counterelites, responsible for social decay at home and dissolution of the empire abroad? Who were these "verbalizers of discontent" (p. 316) who cast "ideologically inspired aspersions on the conduct of both low-level warfare (My Lai) and highest-level political office (Watergate)" (p. 319)? Subversive conspirators? Traitors? Editors of the *Monthly Review?*

They were, writes Liska, "societal elites *farthest removed from the productive process* [my italics], such as college students and professional intellectuals and media men" (p. 319). They were "America's 'responsible' [i.e., liberal] political elites, intellectual dissenters, rationalizing interpreters, and impressionable public. . ." (p. 330). The phraseology and world-view unavoidably recall other such de nunciations, of "nattering nabobs of negati ism" and an "effete corps of intellectu snobs." *Déja vu,* but with one new twist: th very source of those denunciations, a "self-co sciously conservative administration," is itse implicated in the crime of empire-liquidatio because it failed "to seize upon an opportuni to reach over the heads of a dispirited liber political elite and professional middle class t the masses (including the conservative middl class) on the basis of the defense of empire i exchange for disciplining the mode of soci change" (p. 349).

Career of Empire is not a conspiracy theory but it does share the premise of some conspira cy theories—that America must be, at home an abroad, a seamless, imperial web. As such, i provides little guidance in establishing prioritie and allocating appropriate resources—that is, i dealing with the real dangers, which Lisk points out, of a world of infinite problems an finite power.

MARK HELLER

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Patriot or Traitor: The Case of General Mihailovich—Proceedings and Report of the Commission of Inquiry of the Committee for a Fair Trial for Draja Mihailovich. By David Martin. (Stanford, Calif.: Hoover Institution Press, 1978. Pp. xviii + 499. $19.00.)

This book does not unravel the complex question of what happened in Yugoslavia during World War II; it does not pretend to. Its chief value, in my opinion, is in pointing to the inadequacies of works (e.g., Tomasevich, Auty, Barker, et al.) which have aspired to do so. The book consists of two parts: (1) a lengthy introduction, and (2) proceedings and report of the Commission of Inquiry, made up of jurists of national distinction, set up by some 70 prominent Americans who sought a fair trial for Mihailovich, after the Tito regime rejected the U.S. government request that American military personnel who had been in Yugoslavia during the war be allowed to give testimony at the trial.

The commission addressed many probing questions to some 20 Americans, some had been officially attached to Tito's Partisans and Mihailovich's Chetniks, while others were American airmen who had been forced to bail out over Yugoslavia and were rescued by the Chetniks. In some 300 pages of testimony, reproduced here, the commission, which had been interested in evidence of alleged collaboration with the Germans, found no conflict in the testimony of these three classes of witnesses, none of whom had observed or learned of any collaboration, while several had witnessed military actions against the Germans.

While the preservation of the work of the commission in widely available form is to be applauded, I am convinced that the introduction is of far greater importance. For the most part, Martin concentrates on the period (the latter part of 1943) when the British were in the process of abandoning Mihailovich. He makes much use of the Operational Log, largely excerpts of messages to and from British liaison officers with the Mihailovich and Tito forces, maintained by the Special Operations Executive (SOE) in Cairo. In going over the Foreign Office and the prime minister's files on Yugoslavia for the same period, Martin was amazed at how little material from the log was reflected in those files. He produces significant information to indicate that the Yugoslav section of SOE Cairo was dominated by Communists, who deliberately did not pass on to London information favorable to Mihailovich. In addition, SOE prevented the transmission of certain messages from American officers with the Chetniks.

This slanting of intelligence, to which MI 6 also seems to have contributed, was compounded by the pro-Partisan broadcasts of the Yugoslav desk of the BBC, some of which attributed to the Partisans several major Chetnik actions against the Germans in the critical months of late 1943. Requests from British officers with Mihailovich for corrections were ignored, leaving Mihailovich to wonder even about the reliability of these officers, to say nothing of British policy.

The log, from which Martin reproduces (pp. 70–82) a chronology of Chetnik actions against the Germans during the period when they were being abandoned, has been publicly available to scholars since 1974, but has not been quoted from even by those who had access to it (e.g., Auty and Barker). The one-time head of the Southern European Department of the Foreign Office told Martin that had the detailed information on the Chetniks been available to the appropriate quarters in London, it certainly would have had an effect on the decision-making process in the Foreign Office.

Some will ask: would the outcome in Yugoslavia have been any different? No one can say with certainty. But Martin's work makes it evident that much more research is needed if we are to have a reasonably accurate picture of Mihailovich's movement and the policies of the Western allies toward it.

ALEX N. DRAGNICH

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International Tourism: A Political and Social Analysis. By Harry G. Matthews. (Cambridge, Mass.: Schenkman Publishing Co., 1978. Pp. viii + 99. $11.25.)

"The literature of tourism today is fragmented from a social science perspective because there has been little effort to integrate the work of researchers in different fields" (p. 87). The complaint still stands despite Harry Matthews' efforts at meshing a variety of explanatory perspectives in this slim volume. What Matthews does attempt—and he executes this task with commendable clarity—is to suggest the basic parameters for a political investigation of tourism.

The central argument of Matthews' tightly organized study appears to be that because of the *mass* character of international tourism, institutions are appearing that channel and organize this phenomenon in ways that are highly competitive but little understood in terms of their ultimate impact. This becomes a major political problem for both developed and developing societies because of the magnitude of international tourism. The effort to understand the possibilities and pitfalls of tourism is further complicated by the ideological prisms through which tourism is viewed. Still more dangerous is the fact that one of these ideological perspectives, that of corporate capitalism, has become the overwhelming paradigm for an analysis of tourism. "Those who question either the kind or volume of tourism seem to speak in hushed whispers" (p. 91).

Matthews supports his thesis with arresting detail, but his treatment is too brief to cover adequately many of its very intriguing themes. His pluralistic interest-group approach focuses on three levels of tourism politics: the "metropolitan" nation where the bulk of the market exists, the host country, and the spectrum of groups which attempt to influence policy regarding tourism.

Matthews pursues three central questions: who benefits from tourism, at what cost, and how adequately are benefits and costs measured? His answers are tentative but provocative. He finds that government encouragement of tourism constitutes a national subsidy to the travel industry only partially offset by such benefits as taxes and reduction of travel trade deficits. For the host country he adds numerous caveats to such measures as gross foreign exchange earnings and the "multiplier effect," both of which purport to illustrate the importance of tourism to development. These neglect, he argues, the "import-prone" nature of tourism and the problems of accounting remittances when ownership and/or control are in foreign hands. To these costs he also adds the social costs which those marketing tourism seldom acknowledge and which even critics have difficulty estimating.

The picture which emerges may be affected by the fact that Matthews used the U.S. and Canada for his metropolitan nations and Barbados as his host country example. The Caribbean, particularly, has been the focus of many of the critiques of international tourism. However, research on tourism in Pakistan, the Philippines, and other Third-World countries also tends to support his concern that international tourism has become alarmingly homogeneous.

As Matthews illustrates, it threatens to lock the industry into a single marketing format, to depersonalize tourists and trivialize and despoil their destination while preventing the funds generated by tourism from developing the nation's potential or being a stabilizing influence in the society. The villain: an uncritical acceptance of "corporate capitalism" which has inadvertently developed maximally intrusive forms of tourism which immediately threaten tourism's developmental potential and ultimately threaten the industry itself.

The most intriguing social issue raised in the book is perhaps ultimately going to be the most important politically: does tourism need to be so intrusive? Do the fantasy aspects of tourism require the tourist to be so disruptive of the local economy and culture? For all the marketing information and all the social critiques of tourism available, this question is yet to be systematically addressed.

LINDA CLARK RICHTER

Kansas State University

The Origins of the Cold War in Asia. By Yōnosuke Nagai and Akira Iriye. (New York: Columbia University Press, and Tokyo: University of Tokyo Press, 1977. Pp. ix + 448. $25.00.)

This interesting volume is the result of a conference held in Kyoto in 1975, supported through a grant from the Japanese Ministry of Education and designed to bring together American, Japanese, and British scholars to have another look at the development of the cold war, and to make use of recently available documents from the 1940s. It goes without saying that conference volumes are usually uneven, but in this case the unevenness itself provides food for thought. This is because the unevenness has an apparent relationship to the country of origin of each scholar. In general, the American scholars discuss the onset of the cold war within the comfortable confines of the mainstream versus revisionist framework, an American peculiarity if there ever was one; the British historian D. C. Watt provides a distinctive British view in his thoughtful introduction and his chapter on British policy, both of which occasionally take aim at American cold war revisionists yet document the serious divisions between U.S. and British policy that have provided so much grist for the revisionist mill. The Japanese scholars, with only one or two exceptions, would prefer to talk about anything *but* Japan's role in the events in the 1940s—including Poland (see Itō Takayuki's contribution), a curious inclusion in a book ostensibly about Asia. The reader concludes that there may well be a Japanese view on these events, but it won't be found between the covers of this book and instead is avoided at all costs.

The best articles in the book are those by Watt, Akira Iriye, George Kahin, Okabe Tatsumi, Okonogi Masao, and Allen S. Whiting. Whiting says little about the *origins* of the cold war—a problem with many of these essays—but instead uses some of Mao Tse-tung's writings made available during the Cultural Revolution to shed light on Chinese views of the Korean War, the Quemoy crisis of 1958, nuclear weaponry and, most important, the Chinese view of the U.S. Far from seeking confrontation or possessing "worst case" expectations of American behavior, Whiting shows Mao to have held a balanced view of both the dangers and the potentialities of the Sino-American relationship, premised on Mao's belief that the U.S. was a declining power at least by the late 1950s if not earlier, leading to increasing disarray in the Western world. Toward the end of the article Whiting also sets Mao off from the "Gang of Four," showing through creative use of children's booklets that Chiang Ch'ing and others fostered a virulent anti-Americanism in such media that went far beyond Mao's own views.

Iriye's interesting article documents a set of consistent assumptions in American planning for Japan that defined the course of U.S. policy toward Japan throughout the decade of the 1940s. Through careful use of declassified documents, Iriye shows that there was no grand reversal in U.S. policy toward East Asia, from an alliance with China to one with Japan; instead there was "an underlying pattern of continuity" stretching from 1941 to 1949. A corps of Japan specialists in the State Department spewed out reams of papers on postwar Japan from 1941–45, quickly adopting a consistent position, and eventually a consensus, that Japan would be liberalized rather than destroyed, administered under predominant American authority at war's end, such that no other power could make military use of the Japanese islands, and allowed to preserve the emperor system if it aided in the reconstruction of Japan. A reformed Japan would serve general American interests in East Asia far better than a dismantled or a revolutionized Japan. Thus, Iriye finds, "American unilateralism" defined the policy position of these experts from the beginning, and eventually defined the postwar policy toward Japan.

Okabe's essay addresses "changing Chinese Communist perceptions" of international relations in the late 1940s, finding three basic views: an image of cooperation among the Allied powers, held from the war years until mid-1946; an image of the "intermediate zone," a vast region which U.S. imperialism attempted to exploit, continuing to the end of 1947; and a third image of tight East-West bipolar confrontation ensuing thereafter. This essay, seemingly modeled on earlier studies by Iriye (e.g., *Across the Pacific*) which emphasize mutual images and perceptions, is at its best when discussing Chinese theories of the "intermediate zone" and the policy of self-reliance, both based on horizontal polarization of East and West, and vertical polarization between big powers and small nations. Although this article covers familiar ground, Okabe argues shrewdly and exploits a variety of rarely used Chinese sources.

Okonogi Masao's contribution views the Korean War as "basically a domestic conflict over the legitimacy of political power" between two Korean sides, a conflict that both drew in the big powers, and one that the big powers internationalized. Thus, Okonogi argues forcefully, "domestic and international elements were intertwined" in this war. He then proceeds to document his thesis using materials in several languages; the article is fascinating, and tantalizing in its unfortunate brevity.

Robert M. Slusser and John Lewis Gaddis also examine the Korean War and U.S. and Soviet policies toward Korea; both articles are as interesting for their flaws as for their strong points. Slusser treats a difficult and neglected topic: Stalin's goals in Korea. Unfortunately, he is forced to argue by reference to what Stalin did *not* say about his goals, postulating "a chain of pregnant Soviet silences," "a series of meaningful silences" on Stalin's part. Whether it is a silent majority or a mute Joe Stalin, political goals are hard to document in the absence of statement. One goal that Stalin was pregnant with, Slusser says, was a warm-water port in Korea; yet no such baby emerged from the Soviet-occupied womb in northern Korea. Perhaps it was just a glimmer in Uncle Joe's eye? Gaddis, like many other contributors, takes on the revisionists; his presentation of American policy toward Korea in the late 1940s is based for the most part on documents coming out of Washington. Yet without looking at the now-available record of the American occupation in Korea (1945–48) one cannot understand how Washington's ostensible policy for Korea was transformed into something quite different by those on the scene in Seoul. From the Seoul side of things the revisionist arguments appear much more cogent. In reading Gaddis' account one wishes that Korea policy had in fact been an easy matter of Washington enunciating the policy and watching its faithful implementation, instead of continually seeing the best plans done under by Americans and their Korean allies in South Korea. Okonogi is much better on the dialectic between Washington's wishes and the reality of occupation policies.

These are the articles that held my interest. Many of the others—especially those by Nagai, LaFeber, Aruga, Ito, Nakajima, and Yamamoto—either offer little that is new or wander far (often very far) from the ostensible subject of the book. Still, the book will hold the attention of specialists on the cold war in Asia, will be useful in classroom assignments, and all in all constitutes a welcome contribution to the field.

BRUCE CUMINGS

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Armed Struggle in Palestine: A Political-Military Analysis. By Bard E. O'Neill. (Boulder, Colo.: Westview Press, 1978. Pp. xiii + 320. $20.00.)

The main virtue of this rather uneven study of the Palestinian "war" against Israel is the military perspective which Bard O'Neill brings to bear on a subject which is usually approached from historical or partisan positions. O'Neill clearly admires Israel's counter-insurgency efficiency and shows little enthusiasm for the goals or efforts of the Palestine Liberation Organization (PLO), but for the most part he does not allow these feelings to divert him from his course: a methodical assessment of the conflict in terms of a framework of important factors—popular support, organization, unity, external support, the environment, and the government's counter-insurgency role.

The military part of this "political-military" analysis is stronger than the political. O'Neill first sets forth a concise theoretical statement about insurgency in general, drawing upon the major writers, including Mao Tse-tung, Che Guevara, Samuel Huntington, Ted Robert Gurr, and others. Then, after a fairly superficial exploration of "the political context," he moves into the substance of the study by examining Israeli counter-insurgency efforts against the Palestinian *fida'iyin.* Unfortunately, he accepts too unceritically Israel's description of the occupation as "liberal" and tends to give Israel the benefit of the doubt when evaluating the persistent charges of torture and other excesses on the West Bank and Gaza. But he does succeed in providing a sober and straightforward account of Isaael's success in limiting internal organized opposition and containing externally based guerrilla and terrorist activity. O'Neill's comments about the Israeli government's use of "sound counterinsurgency principles" and his discussion of the physical environment, which is on the whole unfavorable to guerrilla warfare, are quite illuminating; and he shows clearly the folly of applying "general" theories of insurgency developed for jungles or rugged, wide-open spaces to the terrain of Palestine.

Perhaps the most hard-hitting part of the book is chapter 5, which deals with the lack of organization and cohesion within the PLO and its main guerrilla organizations. The critique is devastating and should make valuable if unpleasant reading for PLO officials. O'Neill recounts the ideological disagreements, the personal rivalries, and the complicating inter-Arab state interests that accounted for the poorly executed, badly focused and short-lived heyday of Palestinian military activity against Israel from 1968 through Black September 1970, and its decline after that debacle in Jordan to sporadic and senseless acts of violence thousands of miles from Israel. The author succeeds in revealing the enormous limitations affecting any Palestinian military option through his cool description of the failure of the PLO's armed struggle campaign in the late sixties and early seventies.

On the political side, O'Neill concludes that the PLO also largely failed to mobilize popular support among Palestinians, especially in the occupied territories. Only in the refugee camps and among the intellectuals does he find evidence of success in this crucial requirement for effective armed struggle. He also finds that the external support provided by various Arab states to be limited at best. Here I think O'Neill has underestimated the PLO's political capabilities. Most observers of Arab affairs believe that the PLO has won the allegiance and support of Palestinians everywhere (the occupied territories included), particularly since 1974. The non-military infrastructure of the PLO—to which O'Neill devotes only a few paragraphs—is extensive and significant, with its popular organizations, professional societies, factories and financial institutions, medical services, social security program for the families of fighters, and not least its quasi-parliamentary National Council. Perhaps because the study's sources do not reveal direct access to the complex Arab political environment, it tends to underestimate the salience of the Palestinian cause both to Palestinians and other Arabs. Another reason for this misperception is the time frame of the analysis, which deals mainly with the 1967–73 period. Lacking on the one hand a sense of the long history of the conflict and on the other a close analysis of developments since 1973, O'Neill focuses on those years of almost anarchic growth of the movement which were followed so quickly by the disastrous collision with Jordan and tension with other Arab states. Thus he tends to miss the underlying continuity of support which keeps the PLO afloat despite its strategic muddleheadedness and military inefficiency. Indeed, today the PLO is richer, stronger, better organized, and more popular than it was during its explosive adolescence in 1968–70.

But the PLO's prospects for effective armed struggle remain marginal, as O'Neill correctly indicates. In light of this impotence, yet recognizing that it retains at least limited political clout, he concludes his study with the suggestion that a PLO-administered Palestine entity might put to rest the political tension in the Middle East without threatening the security of Israel. This suggestion, and the logic behind it, is provocative, and it would be even more compelling had O'Neill given due weight to the political importance of the PLO. Despite this flaw and others such as repetitiveness, reification of terms like "ideology," and insufficient Arabic sources, this study effectively highlights the Palestinian military dilemma. But it adds considerably less than one might have expected to the excellent earlier works by John Cooley *een March, Black September,* 1973) and by liam Quandt, Fuad Jabber, and Ann Lesch *e Politics of Palestinian Nationalism,* 1973).

MICHAEL C. HUDSON

orgetown University

cision Making in the European Community. By Christoph Sasse, Edouard Poullet, David Coombes, and Gérard Deprez. (New York: Praeger, 1977. Pp. xv + 352. $29.95.)

This book belies its title. It is not an analysis decision-making processes in the European mmunity, but rather a combination of three scriptive and prescriptive pieces on European mmunity institutions. The three sections are ked by a central purpose: "to analyze the derlying causes of the deficiencies in the licy-making process" (p. 138). In pursuit of s diagnostic task, the authors first devote a apter or two in each of the three sections to a ort and, for the most part, derivative descrip- n of the main institutions involved in Euro- an Community affairs. They then go on to fer "practical suggestions for achieving in- ased efficiency" (p. 87).

The opening section by Christoph Sasse on e role of national governments and parlia- ents in Community affairs is somewhat un- en. The Federal Republic, for example, gets a eat deal more attention than the other mem- r states. The materials in the chapter, mostly itten for earlier publication in Germany, uld also use some updating. In sum, there is ly a little here that goes beyond the excellent onographs in the PEP/Chatham House series Martin Niblock (*The EEC; National Parlia- ents in Community Decision Making,* Lon- on: PEP/Chatham House, 1971) and Helen allace (*National Governments and the Euro- an Communities,* London: PEP/Chatham ouse, 1973).

The second section, by Edouard Poullet and erard Deprez, departs from the public law proach of Sasse and examines the structure d functioning of the European Commission om an organizational sociology approach. espite the recognition at the outset that dividual institutions of the Community must viewed as "constituent elements in an teracting system" (p. 133) and the "impossi- lity of analyzing the Community system by ference, explicit or implicit, to any given stitutional model" (p. 134), Poullet and eprez do not really pursue this line of inking. Quite the contrary: there is almost no ference to process and environment or any effort to develop a description of a dynamic institution at work.

David Coombes' reflective chapters on the role of national parliaments and the European Parliament, centered on what he believes to be the crisis of legitimacy in the European Community, provide interesting and constructive insights into legislative processes. Importantly, he points up the Community's failure to acquire political roots of its own, a shortcoming that in all probability will not be made up even by the introduction of direct elections to the European Parliament in June 1979. Coombes' chapters provide a more satisfying conclusion to a book that is for the most part rather short on analysis and insights.

It is disappointing that the authors of this long collection made so little of their opportunities. Not only do they fail to come to grips with the process of decision making in the European Community, but also even their descriptions of the institutional machinery are static and overly legalistic. With only lip service paid to the organic and ever-changing nature of political processes, it is little wonder that the collection has withstood the test of time rather poorly.

Since the prescription stands or falls by the quality and relevance of the description, the authors' remedies for the alleged ills of the Community system are also unconvincing. If indeed, as Sasse suggests, "The European Community is suffering from a discrepancy between achievement and expectation" (p. 113), we cannot help asking the question: whose expectation? It is very doubtful that it is public expectations, as the authors appear to indicate. This of course brings us back to an issue which the authors barely address: the sources of demands and constraints placed on Community institutions.

A similar failure to address crucial questions allows Poullet and Deprez, for example, to suggest that what is necessary for the Commission is that "the decisions taken should correspond to its proposals" (p. 131). Surely one of the most fascinating and unusual features of Community decision making is the very fact that proposals, carrot-like, stand a good way ahead of decisions. Just as Commission memoranda have fulfilled this function since the early 1960s, so also summit meeting communiques now do the same thing.

Perhaps some of these misperceptions stem from the time-worn habit, on the part of scholars and practitioners on both sides of the Atlantic, of assuming that there is a single identifiable phenomenon called "European integration" and then, if such a phenomenon is to be located, that some inherent good is to be found in it. Poullet and Deprez make no bones about this in their prescriptions for the Commission: "It will still need an . . . overall picture of the form of organization that seems to it to be best suited to its essential mission, namely the preparation of Community policies as part of a global and coherent view of the Community integration process" (p. 178). No one is faulting the idealists, but in this world of practical politics and economics, of small improvements and major setbacks, the holy grail of integration and supranationalism emerges as something of an anachronism.

Observers of the European Community now have at their disposal a whole library of informative and useful studies of its institutions, old and new. What is sorely lacking are sensitive anre perceptive analyses of these institutions in action in the day-to-day world of European and national politics.

GLENDA G. ROSENTHAL

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Africa Reports on the Nigerian Crisis: News, Attitudes and Background Information; A Study of Press Performance, Government Attitude to Biafra and Ethno-Political Integration. By Henryka Schabowska and Ulf Himmelstrand. (Uppsala: Scandinavian Institute of African Studies, 1978. Pp. 161. Skr. 75.)

The lengthy and complex title of this study indicates its importance, conclusions, and deficiencies. The effort to address the previously unexamined role of the African press in one of the continent's great conflicts is welcome, particularly in the context of the present battle within UNESCO over the role of the press in the Third World. The sponsoring institute has generally offered monographs of descriptive and theoretical distinction. Unfortunately, this is not one of them.

The work comprises three parts: two chapters on the general approach to mass media in developing countries and the specific aims of this research; two chapters on the history, tradition, and structure of African press and news agencies; and two chapters on attitudes of African states to the Nigerian civil war—as seen in the press of Ghana, Tanzania, Senegal, Ivory Coast, and Guinea, selected to represent pro-West and pro-East, anglophone and francophone, and pro-federation and pro-Biafran states. Eleven publications were analyzed during four stages of the conflict: January-September 1967, Aburi and secession; May-Octob 1968, peace talks; May-September 1969, Ahia Declaration and stalemate; and Decembe February 1969–1970, war's end. The h pothesis, claimed to be confirmed, is that "tl views of African governments were reflected in the press, and these in turn were shaped t "the degree of ethno-political national integr tion" (pp. 10, 11). These two propositions an their connection may well be valid; but tl verdict is "not proven," owing to serious—an in my opinion, fatal—flaws in theoretical unde pinnings, research design and techniques, an presentation.

The initial ventures into theory about ma media and the developing world are a jumble (ideas, diagrams, and models, most of which ai then used unconvincingly—or not at all. Vit factors in African policy towards Biafra, such the relationship of various governments t France and to DeGaulle, or the regional comp tition between Ivory Coast and Nigeria, ai never mentioned. The choice of ethno-politic integration as the controlling factor in policy i thus not justified; assuming that it were, othe states (such as Sierra Leone) should have bee considered as well.

The research itself was at best careless, i both general and Africanist terms. There ar great gaps in the materials, attributed to th inability to get complete files, which th authors claim—unconvincingly—do not damag the findings (pp. 66–68). If the needed materi als were not obtainable, the study should hav been terminated or redesigned. Other mis directed choices or simple errors of fact furthe undermine even descriptive utility (for exam ple: the use of the *Ghanaian Times,* always state-owned paper despite the identification t the contrary, rather than the *Daily Graphic* with by far the largest circulation in Ghana, an the overemphasis on the importance of th Catholic-oriented *Afrique Nouvelle* in the over whelmingly Muslim society of Senegal). Th ability of coders whose first language is neithe English nor French, and who are (apparently not Africanists, to fully understand the impor and significance of reports and editorials in th African press can be questioned. The codin scheme itself is not presented and must b obtained by writing to the authors! (p. 68).

These shortcomings of content are matched by a didactic and ponderous style. Documentation is thin, and many footnotes (e.g., nn. 3 and 6 of chapter 2) bear no obvious relevance to the text. The results of translation from the original Polish of the principal author should have been more carefully checked. In a work of greater theoretical strength, such unevenness would perhaps be of lesser importance; but here, particularly as the subject itself is the written word, it is unforgiveable. Given the price of this book, the student of Africa and of the media would be advised to await some other study.

MARSHALL CARTER

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Anti-Apartheid: Transnational Conflict and Western Policy in the Liberation of South Africa. By George W. Shepherd, Jr. (Westport, Conn.: Greenwood Press, 1977. Pp. xii + 246. $15.95.)

During the past two decades, the international political system has been significantly transformed by the emergence of myriad transnational actors. Most prominent among them are, of course, the multinational corporations, whose role and impact have been the object of study and controversy by both economists and political scientists. Nonetheless, another category of transnational actor has also altered the dynamics of the international political system—the non-governmental or voluntary organization that has extended its influence into the national liberation struggles and the struggle for the promotion and protection of internationally recognized human rights standards. This transnational actor—the NGO—has, however, not yet attracted the serious analysis that it warrants. For this reason, George Shepherd's *Anti-Apartheid,* with its focus on "the conflict-oriented transnational human-rights actors working against apartheid" (p. 12), is a noteworthy and important contribution to international relations theory, in addition to further illuminating the anti-apartheid struggle and the forces sustaining racism in southern Africa.

Shepherd perceives the anti-apartheid struggle as a continuation of the nineteenth-century abolitionist movement, in that apartheid is considered a form of slavery (forced labor). The main resistance to the abolitionist movement is *"the Atlantic system,"* a concept drawn from regional and dependency theory (p. 11), whose core powers are the industrialized Western nations (the United States, the United Kingdom, Germany and France). South Africa is seen as a semi-peripheral power of the system, providing essential services of the core and helping the Atlantic capitalist powers penetrate economically the rest of the Southern African periphery. To bring about the defeat of apartheid, it is necessary to destroy this symbiotic relationship.

It is in this context that Shepherd examines the role and the effectiveness of the numerous NGOs that comprise the anti-apartheid movement, with particular emphasis on American and British organizations. In the course of his analysis, Shepherd provides a wealth of descriptive material on the origins and nature of these transnational actors—an invaluable contribution in and of itself; however, the concern of his study is the policy results of these human rights NGOs, not the organizations themselves. Specifically, he examines the extent to which NGOs contribute to the growth of transnational abolition pressures on apartheid—through education, support aid, and the strength of the liberation and majority movements. Chapter 2 details the manner in which the human rights NGOs have been able to assist the United Nations process of identifying injustice and recognizing the legitimate representatives of the powerless people; chapter 3 traces the development of the human rights movement from an initial protest period of legalistic focus on reform of law, through a period of direct action, to the current stage of direct support for the liberation struggle; chapter 4 analyzes the trends in both the U.S. and the U.K. among black NGOs, away from assimilation and co-optation and toward black consciousness and multiracial cooperation; and chapters 5 through 7 examine the campaigns for an arms embargo, assistance to liberation movements, and transnational sanctions (including the bank campaign, the sports boycott, and economic boycott) that have been waged in recent years.

Throughout his analysis, Shepherd is conscious of—and seeks to illuminate—the splits and factions within the anti-apartheid movement, particularly between the "gradualist" strategy, in the direction of domestic reforms with minimal external pressure, and the "abolitionist" strategy, in favor of disengagement and support for liberation. He, himself, is convinced—and convincingly argues—that only the latter holds hope for eroding the structure of apartheid:

> One of the assumptions . . . of this study is that fundamental solutions to racial conflict emerge out of racial class changes rather than out of individual attitudinal changes. A change in attitude may facilitate change, but it cannot substitute for changes in the power position of groups (p. 204).

Under the enormous pressures for changes generated by the external linkages which have been forged by NGOs with the internal racial complex in South Africa, the South African government has made limited adaptations: the sacrificing of "petty apartheid," the initiation of "detente" and "dialogue" with Black African states; the "homelands" policy; and some concessions to the Black Consciousness and trade-union movement, followed by even more repression. But, none of this has altered the basic stratification. Only revolution will bring down the racist structure. Moreover, states Shepherd, "The revolutionary option is real and is being facilitated by the abolitionist movement abroad. It is time that more people saw clearly the conflicts between the abolitionists and the gradualists and began to make some hard choices" (p. 226).

Shepherd's conclusions suggest that the NGOs have not only played a vital role in bringing the southern African situation to the point where "the revolutionary option is real," but that in certain respects, "they play a formative role in world politics" that is occasionally "more determinative than that of major powers" (p. 13):

> Western Governments may well continue to support the apartheid superstructure. But its collapse has already begun and will continue until the persistent nongovernmental actors, along with the liberation movements, secure fundamental change (p. 229).

LAURIE S. WISEBERG

Human Rights Internet

The Politics of International Economic Relations. By Joan Edelman Spero. (New York: St. Martin's Press, 1977. Pp. vii + 326. $12.95, cloth; $5.95, paper.)

The appearance of this volume is to be welcomed enthusiastically by a discipline that is trying to make sense of the growing prominence of economic issues in American foreign policy and international politics. Theoretically, it reflects a growing disciplinary tendency to break the artificial bonds imposed by "realism" on international politics with its assumptions of a single issue-system governed by an elite of national governments preoccupied by the ebb and flow of "power" narrowly defined. Spero restores the inherent connection between international economics and politics, artificially severed during the postwar dominance of international relations scholarship by realism; she argues persuasively that *in reality* the uncoupling of the two spheres was never valid. Along with several recent works—notably, Keohane and Nye, *Power and Interdependence* (1977) and Blake and Walters, *The Politics of Global Economic Relations* (1976)—this book provides a concise appreciation of what is meant by fashionable but ill-used concepts like "interdependence" both among actors and among issues.

Spero has organized her introduction, ten chapters and conclusion around three issue-areas—a Western system, a North-South system and an East-West system. She identifies within each the key issues and their peculiarities, the key participant actors and their roles, how these issues and the systems they constitute have been organized and administered. Treated historically, each issue (i.e., "international monetary management" within the Western system or "the use of aid" within the North-South system) is presented in sufficient technical depth to avoid even the slightest hint of superficiality yet so as to strengthen inexorably but subtly the core argument that "international economic relations, in and of themselves, constitute political relations" (p. 9). The material is presented so that no single issue or system appears to function in isolation from the others. We are constantly reawakened to the links among them, the changes in those links and their interplay with conventional concerns of "peace and power." The discussion of each issue manages to provide both rich description and compelling theoretical insight, so that there is something of value in every section for professional economists interested in the relevance of their concerns to political outcomes and political scientists seeking to acquire a grasp of the intricacies of economic phenomena and to integrate them.

If Spero's facility for explaining the nuts and bolts of economic questions will delight political scientists and students, so too will her capacity for explaining the political nuts and bolts of these questions. The impact of bureaucratic contention, both within and across national frontiers, is elaborated, and, by inference, we are alerted to the unhappy consequences of what Steinbruner calls "cybernetic" decision making. In addition, the permanent bond between domestic and international politics (and economics) and its reciprocal consequences are consistently dealt with and explained.

Among the book's most important themes are those concerning the decline of American world influence since the period of virtual politico-economic hegemony immediately after World War II, the shifting nature of global cleavages since this period and the growing enfeeblement of "sovereign" actors trying to cope with international economics by self-help. The first is highlighted in the Western economic system, and in the process Spero helps to clarify the almost linear decline of American olitical influence that has apparently beildered many students and practitioners of iternational affairs since Vietnam and the iiddle East oil embargo. The second theme is oven with the first as we are shown how the merican decline and the growing challenge of 'estern Europe and Japan, mirrored by the atus of the dollar and contention over marets, have ended America's politico-economic magnanimity" and have produced increasing iction with friends and allies. Such growing ontention and the tasks attendant upon it are laborated in the author's descriptions of the il question (to the point where allegations are resented concerning American-Arab collusion ı the initiation of OPEC and the raising of oil rices), the Nixon "shocks" (including pressure n Japan and unilateral devaluation), and the rowing competition between the U.S. and ther multinationals for markets in the Soviet loc. This last issue also serves to reveal the rowing gap between the perceived interests of merican business and government. Whether he subject is the role of multinationals, the roblem of cartels or the dilemma of petrodolars, the theme that national autonomy is ncreasingly and inexorably being undermined, nost dramatically in the case of the United tates, is returned to again and again.

In sum, this is a volume to elate teachers eeking a text to explain the "new" phenomena 'et which serves as an excellent first source for cholars who desire a broadly informative, vell-written and theoretically valuable framevork for reintegrating international politics and conomics.

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nalyzing Soviet Strategic Arms Decisions. By Karl F. Spielmann. (Boulder, Colo.: Westview Press, 1978. Pp. xv + 184. $16.00.)

This is not a book about Soviet strategic ower, or even about Soviet decision making on trategic arms. As the title says, it is a book bout *analyzing* Soviet arms decisions. Readers vho finish it may know not much more about oviet strategic power or about Soviet strategic rms decisions than when they began it. They vill have learned instead that those who anayze Soviet strategic arms decisions—presumaby not the readers themselves: there are not nany such analysts and Spielmann assumes that hey must work inside the government where he requisite classified information is—should ot attempt to analyze them at a single go. "Multiple approaches," as advocated by Graham Allison (see below), are needed. The analyst should attack the subject three times, using: a rational strategic-actor approach; a pluralistic decision-making approach; and a third approach which postulates that the top Soviet leaders in making strategic arms decisions take account of diverse factors, not just strategic considerations.

Why three *distinct* approaches instead of a combined approach? Because a combined approach would "lump a variety of assumptions together within a single analytic framework" (p. 11). The distinctiveness of discrete approaches "is likely to be compromised by merely combining them. It would be difficult to prevent the new unified theory" from being biased to favor the view that Soviet strategic decision making is rational or, alternatively, that it is determined by bureaucratic struggle. Spielmann acknowledges that in a combined approach diverse "factors may well be taken into account, but only sufficiently so to assure skeptics that the analyst had been broadminded" (p. 11). Spielmann is so strongly persuaded himself of the difficulties of a unified approach that he hardly pauses to persuade the reader that they are indeed insuperable.

Spielmann does not promise much in the way of positive knowledge to analysts who follow his prescriptions, and still less to policy makers who employ analysts to help them understand the Soviet strategic threat so that they can decide how the United States ought to respond to it. Preoccupied as he is with the advancement of the methodology of analyzing Soviet strategic decisions, Spielmann neglects to show how the multiple approach would provide United States policy makers with better judgments about the motives and intentions that underlie Soviet strategic programs. In fact, he acknowledges that the policy maker is not interested in receiving "different answers to the same question, and it is more difficult to make an overall judgment when multiple approaches are used" (p. 10). The chief virtue claimed for the multiple approach to Soviet strategic decisions, it would appear, is that it compels analysts to confront their assumptions and take account of relevant information (p. 143)—that is to say, the method *forces* them to do what scholarship merely *requires*.

Despite the dubious fruits of multiple approaches, Spielmann is not daunted. He concludes with a call for case studies of an array of Soviet strategic arms decisions, admittedly "an enormous undertaking" (p. 142). What are the prospects? As yet, apparently no one has completed a study of a Soviet strategic arms decision employing a multiple approach. Spielmann's mentor, Graham Allison, who employed the method in a book-length work (*Essence of Decision: Explaining the Cuban Missile Crisis* [Boston: Little, Brown, 1971]), acknowledges that he had only illustrated the use of multiple approaches and had not provided a confident account of what had happened (see Allison, pp. viii and ix). Spielmann himself, after a mere 25 pages (around one-sixth of the book) devoted to the first-generation Soviet ICBM program (SS-6), concludes that the task of analyzing the SS-6 decisions along multi-approach lines basically remains to be done (p. 134).

While the method Spielmann advocates here is formalistic and, perhaps, finally barren, he himself is superior to that method. He has published serious substantive articles on the Soviet defense establishment and in this work also makes in passing important points. This is particularly true when he discusses "the pluralistic decision-making approach," a combination of Allison's organizational and bureaucratic models. Spielmann argues persuasively that large interest groups in the USSR—e.g., the military *services*—while they might be politically effective if they were unified actors, actually are heterogeneous and subject to powerful intra-service rivalries; component groups within the services, while they may pursue their interests united and with fervor, do not have great political weight. Spielmann also rightly emphasizes that the Soviet leadership may take account of various group interests for reasons of its own, not necessarily because of group *pressures.* I, at any rate, am unimpressed by the sanctions that might be available to "pressure groups" in the USSR to compel the leadership to adopt decisions they otherwise would not.

MYRON RUSH

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Sanctions: The Case of Rhodesia. By Harry R. Strack. (Syracuse, N.Y.: Syracuse University Press, 1978. Pp. xvii + 296. $15.00.)

Fourteen years have passed since the white minority government of Rhodesia unilaterally declared its independence from the United Kingdom, its metropolitan power. Like the League of Nations sanctions against Italy in the Ethiopian case, the United Nations action imposing enforcement measures under Article 41 of the Charter in order to bring about the downfall of the Smith regime has provided political scientists with an excellent case stud of the effectiveness of sanctions as a tool of th collective security system and an instrument c foreign policy in general. A review of th literature and U.N. resolutions on the subjec allows the conclusion that the enforcemen measures have failed to bring about a solutio of the Rhodesian problem acceptable to th organization that had ordered them. Harry R Strack tends to agree with the view that th Rhodesian sanctions have not been sufficient t isolate Rhodesia from international relations s as to provoke an internal political chang satisfactory to the party imposing the sanctions. Since Strack's book covers the events a of the end of 1977, one might add that th "internal settlement" of 1978 does not seem t be considered by the United Nations as development restoring international peace an security and thereby warranting lifting th sanctions.

Strack's primary purpose is not to analyz the effect of the sanctions on Rhodesia' economy, the approach taken by most commentators, but to identify, explain, and determine how and under what circumstances th Smith regime has been able to avoid isolatio from the rest of the international system, t maintain and even develop its internationa contacts, and to survive as a de facto stat though legally not recognized even by Sout Africa. Strack approaches the problem in most comprehensive way going beyond it economic aspect, although obviously the economic relations occupy about one-third of th text.

Structurally the eight chapters of the book (leaving aside the introduction) can be divided into three parts. Chapters 1 and 2 respectively deal with Rhodesia's international status before the Unilateral Declaration of Independence and the nature and purposes of the sanctions. Against the background of the theory o sanctions Strack traces the application of the enforcement measures against Rhodesia and concludes that, without necessarily being a useful device to induce compliance, sanctions may even be disfunctional by enhancing solidarity in the target state and forcing it to diversify its economy. Strack judges the gradual escalation of the Rhodesian sanctions to be unsound since it gave the Smith regime time to make necessary adjustments. In addition, the confusion prevailing as to the purposes and termination of the sanctions had also affected the adherence to sanctions policy.

The main part of the study meticulously describes Rhodesia's international relations in political and diplomatic matters, including formal and informal links; in economics, the least successful from the enforcement point of view; in communication and transportation; in tourism, labor, and migration; and in social relations, including sport (the most diligently applied sanctions!), education, broadcasting, and general entertainment. Despite obvious difficulties, such as the secrecy surrounding the methods of circumventing the sanctions, Strack, who conducted field research in the area, offers the reader many fascinating details on the Rhodesian government's methods of maintaining sufficient international contacts to ensure the survival of the regime. Strack analyzes the efficacy of the Rhodesian sanctions in all the aspects of international relations within the framework of three perspectives or levels of analysis: global, regional (Southern African), and national. Utilizing the concept of transnational relations, he proves with the example of Rhodesia how difficult it is to isolate a country from the rest of the international system in which so much of intersocietal intercourse involving all kinds of transactions occurs outside of government control. Moreover, the governments are sometimes unable or, even worse, unwilling to control these transactions and actors, such as transnational corporations, banks, trading companies, brokers, and shipping lines. Strack finds this phenomenon particularly conspicuous in the global arena. From the regional point of view Rhodesia's environment has reduced its susceptibility to isolation. Even after the decolonization of Portugal, the states of Southern Africa are so tied together by transportation, economic, and communication linkages (Malawi, Zambia, for example) that the effectiveness of the sanctions against Rhodesia must be reduced if they cannot be applied to the whole region. At the national level of analysis Strack approaches the sanctions from the perspective of the target state's policies designed to counteract the sanctions and the perspective of other national and transnational actors engaged in relations with Rhodesia. A case study of the Byrd Amendment in chapter 4 is of particular interest to the American reader.

The conclusions (chapter 8) sum up the evidence recorded in the main body of the book. Strack's general conclusion is that, leaving aside their symbolic and ideological impact, the United Nations measures have failed to isolate Rhodesia, which casts some doubt upon the efficacy of sanctions in any future application by the U.N. or some other, perhaps regional, organization. Strack believes that the fact that Smith was finally persuaded to accept the principle of majority rule is due primarily to the coup in Portugal in 1974 and decolonization of Portuguese Africa.

Notes, a bibliography (only of English-language sources), an index, a map of the Southern African railroad network, and 15 tables illustrate and complement the thorough analysis in the text.

Strack's well-researched and comprehensive book must be regarded as the definitive study of Rhodesia's efforts to circumvent the effects of the United Nations sanctions. Moreover, it goes beyond being a mere case study by raising within its conceptual framework broader issues, such as interdependence in today's world, the role of transnational actors, and the ability of an international organization to enforce its decisions. In this sense it should interest not only students of international organization or African affairs but also international relations scholars analyzing the fundamental structure of the present international system.

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The "Dollar Drain" and American Forces in Germany: Managing the Political Economics of Alliance. By Gregory F. Treverton. (Athens: Ohio University Press, 1978. Pp. xvi + 226. $12.00.)

With the obvious exception of the Soviet Union, no country in the world is more crucial to the successful pursuit of American foreign policy goals than West Germany. But after 30 years of extraordinarily intimate collaboration, the two allies find themselves at a crossroads in their relationship. On the one side, traditional elements of American dominance in the Association remain—on the U.S. commitment to West Berlin, America's broader willingness to defend West Germany from potential Soviet aggression, and, even more generally, Bonn's expectation that Washington will continue to define the larger purposes of the Western Alliance. On the other side, however, the American economic and energy dilemma places West Germany in a new position of partial—but not full—equality within the bilateral relationship. Indeed, West German leaders publicly maintain that their country is not in the position to assume "joint" leadership of the Western Alliance with the United States; even a joint entente with France is considered premature and politically unwise from the vantage point of West Germany's leaders. Thirty years of political development have nonetheless made the Federal Republic of Germany a latent political force with which the United States must realistically deal.

Gregory F. Treverton's study of German-American conflict over the Troop Offset issue in 1966–1967 sheds interesting light on what, in retrospect, now seems a crucial stage in the political emergence of West Germany. This arrangement called for the Bonn government to purchase an agreed amount of U.S. military equipment in exchange for the stationing of U.S. troops in the Federal Republic. In view of their geostrategic positions, Treverton hardly surprises the reader with the assertion that the two allies had rather different views on the offset issue.

On the American side, the Johnson administration apparently believed that Washington was in the position to exert effective pressure on the soon-to-be-deposed Erhard government. As Treverton insightfully observes, Washington's strategy toward Bonn was simultaneously shrewd and straightforward: "In effect," concludes the author, "the United States used a domain where it perceived itself to be relatively strong—military security—to exact advantage in an area where it felt relatively weak—international finance. Germans (and other Europeans) were presumed to care more about the presence of the troops than the United States. . ." (p. 198). A face-saving agreement was eventually reached with the successor government of Kurt-Georg Kiesinger, and in this very limited sense the Americans got their way. But over the longer term, the confrontation over troop-offset undoubtedly emboldened future German leaders to take a tougher, and more exclusively national, line in dealing with Washington. But why would the Germans even wish to argue with the U.S. over the issue of American forces in their country?

On the German side, Treverton maintains that Bonn viewed American troops in the Federal Republic from two different vantage points. True, an American military presence on German soil continued to enjoy bipartisan political support within the Federal Republic throughout the 1960s. Yet German political leaders were also becoming annoyed with American threats to withdraw U.S. troops in the absence of a German willingness to pay for the stationing of American forces in their country. From Bonn's vantage point, this position was based on a very limited American conception of monetary interest. Had the global Soviet threat receded to the point where only the Germans had something to fear?

Within the American and German foreign policy establishments, Treverton describes and analyzes a fascinating series of bureaucratic struggles. These struggles not only ended up pitting the two national bureaucracies against each other, but different parts of respective government agencies also found themselves in conflict with each other as well: As a case study in the politics of alliance management, Treverton's work reaffirms earlier findings (principally those of Neustadt and Allison) that national governments are anything but monolithic entities.

The "Dollar Drain" has one basic flaw, despite its overall value for students of alliance politics: far too little attention is given to the broader strategic environment in which this important conflict is played out. What impact did DeGaulle's withdrawal from NATO have on the Washington-Bonn association? Did the delicate strategic balance between Moscow and Washington influence the perceptions of American and German negotiators? In fairness to Treverton, these and other considerations are mentioned in his book; but the author could have gone further in exploring how one specific case study related itself to these larger issues. Most case studies are enhanced by the addition of larger strategic factors, and Treverton's study is hardly an exception to that rule. Having said this, however, I must add that no one interested in the present course of German-American relations should ignore this book.

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